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ART. XV.—*Review of Darwin's Theory on the Origin of Species by means of Natural Selection.**

FULLY to understand the foregoing Essay of Dr. Hooker,† it should be read in the light of Mr. Darwin's book. The Essay is a trial of the Theory,—an attempt by one inclined in its favor to see how the theory will work, when applied to the flora of a large and most peculiar province of the world.

This book is already exciting much attention. Two American editions are announced, through which it will become familiar to many of our readers, before these pages are issued. An abstract of the argument,—for "the whole volume is one long argument," as the author states,—is unnecessary in such a case; and it would be difficult to give by detached extracts. For the volume itself is an abstract, a prodromus of a detailed work upon which the author has been laboring for twenty years, and which "will take two or three more years to complete." It is exceedingly compact; and although useful summaries are ap-

* *On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN, M.A., Fellow of the Royal, Geological, Linnæan, etc. Societies, Author of "Journal of Researches during H. M. S. Beagle's Voyage round the World." London: John Murray. 1859. pp. 502, post 8vo.

† This article was intended to follow the remaining part of the essay of Dr. Hooker, commenced in our January number; the continuation of which we are obliged to defer, for want of room.—EDS.

pended to the several chapters, and a general recapitulation contains the essence of the whole, yet much of the aroma escapes in the treble distillation, or is so concentrated that the flavor is lost to the general, or even to the scientific reader. The volume itself,—the proof-spirit—is just condensed enough for its purpose. It will be far more widely read, and perhaps will make deeper impression than the elaborate work might have done, with its full details of the facts upon which the author's sweeping conclusions have been grounded. At least it is a more readable book: but all the facts that can be mustered in favor of the theory are still likely to be needed.

Who, upon a single perusal, shall pass judgment upon a work like this, to which twenty of the best years of the life of a most able naturalist have been devoted? And who among those naturalists who hold a position that entitles them to pronounce summarily upon the subject, can be expected to divest himself for the nonce of the influence or received and favorite systems? In fact, the controversy now opened is not likely to be settled in an off-hand way, nor it is desirable that it should be. A spirited conflict among opinions of every grade must ensue, which,—to borrow an illustration from the doctrine of the book before us—may be likened to the conflict in nature among races in the struggle for life, which Mr. Darwin describes; through which the views most favored by facts will be developed and tested by 'Natural Selection,' the weaker ones be destroyed in the process, and the strongest in the long run alone survive.

The duty of reviewing this volume in the American Journal of Science would naturally devolve upon the principal Editor, whose wide observation and profound knowledge of various departments of natural history, as well as of geology, particularly qualify him for the task. But he has been obliged to lay aside his pen, and to seek in distant lands the entire repose from scientific labor so essential to the restoration of his health,—a consummation devoutly to be wished, and confidently to be expected. Interested as Mr. Dana would be in this volume, he could not be expected to accept its doctrine. Views so idealistic as those upon which his "Thoughts upon Species"* are grounded, will not harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin. Though it is just possible that one who regards the kinds of elementary matter, such as oxygen and hydrogen, and the definite compounds of these elementary matters, and their compounds again, in the mineral kingdom, as constituting species, in the same sense, fundamentally, as that of animal and vegetable species, might admit an evolution of one species from another in the latter as well as the former case.

* Article in this Journal, vol. xxiv, p. 305.

Between the doctrines of this volume and those of the other great Naturalist whose name adorns the title-page of this Journal, the widest divergence appears. It is interesting to contrast the two, and, indeed, is necessary to our purpose; for this contrast brings out most prominently, and sets in strongest light and shade the main features of the theory of the origination of species by means of Natural Selection.

The ordinary and generally received view assumes the independent, specific creation of each kind of plant and animal in a primitive stock, which reproduces its like from generation to generation, and so continues the species.* Taking the idea of species from this perennial succession of essentially similar individuals, the chain is logically traceable back to a local origin in a single stock, a single pair, or a single individual, from which all the individuals composing the species have proceeded by natural generation. Although the similarity of progeny to parent is fundamental in the conception of species, yet the likeness is by no means absolute: all species vary more or less, and some vary remarkably—partly from the influence of altered circumstances, and partly (and more really) from unknown constitutional causes which altered conditions favor rather than originate. But these variations are supposed to be mere oscillations from a normal state, and in Nature to be limited if not transitory; so that the primordial differences between species and species at their beginning have not been effaced, nor largely obscured, by blending through variation. Consequently, whenever two reputed species are found to blend in nature through a series of intermediate forms, community of origin is inferred, and all the forms, however diverse, are held to belong to one species. Moreover, since bisexuality is the rule in nature (which is practically carried out, in the long run, far more generally than has been suspected), and the heritable qualities of two distinct individuals are mingled in the offspring, it is supposed that the general sterility of hybrid progeny, interposes an effectual barrier against the blending of the original species by crossing.

From this generally accepted view the well-known theory of Agassiz and the recent one of Darwin diverge in exactly opposite directions.

That of Agassiz differs fundamentally from the ordinary view only in this, that it discards the idea of a common descent as the real bond of union among the individuals of a species, and also the idea of a local origin,—supposing, instead, that each species originated simultaneously, generally speaking over the whole geographical area it now occupies or has occupied, and

* "Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes." —*Linn. Phil. Bot.*, 99, 157.

in perhaps as many individuals as it numbered at any subsequent period.

Mr. Darwin, on the other hand, holds the orthodox view of the descent of all the individuals of a species not only from a local birth-place, but from a single ancestor or pair; and that each species has extended and established itself, through natural agencies, wherever it could; so that the actual geographical distribution of any species is by no means a primordial arrangement, but a natural result. He goes farther, and this volume is a protracted argument intended to prove that the species we recognize have not been independently created, as such, but have descended, like varieties, from other species. Varieties, on this view, are incipient or possible species: species are varieties of a larger growth and a wider and earlier divergence from the parent stock: the difference is one of degree, not of kind.

The ordinary view—rendering unto Cæsar the things that are Cæsar's—looks to natural agencies for the actual distribution and perpetuation of species, to a supernatural for their origin.

The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin, as equally derivative, equally natural.

The theory of Agassiz, referring as it does the phenomena both of origin and distribution directly to the Divine will,—thus removing the latter with the former out of the domain of inductive science (in which efficient cause is not the first, but the last word),—may be said to be theistic to excess. The contrasted theory is not open to this objection. Studying the facts and phenomena in reference to proximate causes, and endeavoring to trace back the series of cause and effect as far as possible, Darwin's aim and processes are strictly scientific, and his endeavor, whether successful or futile, must be regarded as a legitimate attempt to extend the domain of natural or physical science. For though it well may be that "organic forms have no physical or secondary cause," yet this can be proved only indirectly, by the failure of every attempt to refer the phenomena in question to causal laws. But, however originated, and whatever be thought of Mr. Darwin's arduous undertaking in this respect, it is certain that plants and animals are subject from their birth to physical influences, to which they have to accommodate themselves as they can. How literally they are "born to trouble," and how incessant and severe the struggle for life generally is, the present volume graphically describes. Few will deny that such influences must have gravely affected the range and the association of individuals and species on the earth's surface. Mr. Darwin thinks that, acting upon an inherent predisposition to vary, they have sufficed even to modify

the species themselves and produce the present diversity. Mr. Agassiz believes that they have not even affected the geographical range and the actual association of species, still less their forms; but that every adaptation of species to climate and of species to species is as aboriginal, and therefore as inexplicable, as are the organic forms themselves.

Who shall decide between such extreme views so ably maintained on either hand, and say how much of truth there may be in each? The present reviewer has not the presumption to undertake such a task. Having no prepossession in favor of naturalistic theories, but struck with the eminent ability of Mr. Darwin's work, and charmed with its fairness, our humbler duty will be performed if, laying aside prejudice as much as we can, we shall succeed in giving a fair account of its method and argument, offering by the way a few suggestions, such as might occur to any naturalist of an inquiring mind. An editorial character for this article must in justice be disclaimed. The plural pronoun is employed not to give editorial weight, but to avoid even the appearance of egotism, and also the circumlocution which attends a rigorous adherence to the impersonal style.

We have contrasted these two extremely divergent theories, in their broad statements. It must not be inferred that they have no points nor ultimate results in common.

In the first place they practically agree in upsetting, each in its own way, the generally received definition of species, and in sweeping away the ground of their objective existence in Nature. The orthodox conception of species is that of lineal descent: all the descendants of a common parent, and no other, constitute a species; they have a certain identity because of their descent, by which they are supposed to be recognizable. So naturalists had a distinct idea of what they meant by the term species, and a practical rule, which was hardly the less useful because difficult to apply in many cases, and because its application was indirect,—that is, the community of origin had to be inferred from the likeness; that degree of similarity, and that only, being held to be conspecific which could be shown or reasonably inferred to be compatible with a common origin. And the usual concurrence of the whole body of naturalists (having the same data before them) as to what forms are species attests the value of the rule, and also indicates some real foundation for it in nature. But if species were created in numberless individuals over broad spaces of territory, these individuals are connected only in idea, and species differ from varieties on the one hand and from genera, tribes, &c. on the other only in degree; and no obvious natural reason remains for fixing upon this or that degree as specific, at least no natural standard, by which the opinions of different naturalists may be correlated.

Species upon this view are enduring, but subjective and ideal. Any three or more of the human races, for example, are species or not species, according to the bent of the naturalist's mind. Darwin's theory brings us the other way to the same result. In his view, not only all the individuals of a species are descendants of a common parent but of all the related species also. Affinity, relationship, all the terms which naturalists use figuratively to express an underived, unexplained resemblance among species, have a literal meaning upon Darwin's system, which they little suspected, namely, that of inheritance. Varieties are the latest offshoots of the genealogical tree in "an unlineal" order; species, those of an earlier date, but of no definite distinction; genera, more ancient species, and so on. The human races, upon this view likewise may or may not be species according to the notions of each naturalist as to what differences are specific: but, if not species already, those races that last long enough are sure to become so. It is only a question of time.

How well the simile of a genealogical tree illustrates the main ideas of Darwin's theory the following extract from the summary of the fourth chapter shows.

"It is a truly wonderful fact,—the wonder of which we are apt to overlook from familiarity—that all animals and all plants throughout all time and space should be related to each other in group subordinate to group, in the manner which we everywhere behold—namely, varieties of the same species most closely related together, species of the same genus less closely and unequally related together, forming sections and subgenera, species of distinct genera much less closely related, and genera related in different degrees, forming sub-families, families, orders, subclasses, and classes. The several subordinate groups in any class cannot be ranked in a single file, but seem rather to be clustered round points, and these round other points, and so on in almost endless cycles. On the view that each species has been independently created, I can see no explanation of this great fact in the classification of all organic beings; but, to the best of my judgment, it is explained through inheritance and the complex action of natural selection, entailing extinction and divergence of character, as we have seen illustrated in the diagram.

"The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during each former year may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have tried to overmaster other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was small, budding twigs; and this connexion of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs which

flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear all the other branches; so with the species which lived during long-past geological periods, very few now have living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these lost branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only from having been found in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favored and is still alive on its summit, so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever branching and beautiful ramifications."

It may also be noted that there is a significant correspondence between the rival theories as to the main facts employed. Apparently every capital fact in the one view is a capital fact in the other. The difference is in the interpretation. To run the parallel ready made to our hands:*

"The simultaneous existence of the most diversified types under identical circumstances, ... the repetition of similar types under the most diversified circumstances, ... the unity of plan in otherwise highly diversified types of animals, ... the correspondence, now generally known as special homologies, in the details of structure otherwise entirely disconnected, down to the most minute peculiarities, ... the various degrees and different kinds of relationship among animals which [apparently] can have no genealogical connection, ... the simultaneous existence in the earliest geological periods....of representatives of all the great types of the animal kingdom, ... the gradation based upon complications of structure which may be traced among animals built upon the same plan; the distribution of some types over the most extensive range of surface of the globe, while others are limited to particular geographical areas, ... the identity of structures of these types, notwithstanding their wide geographical distribution, ... the community of structure in certain respects of animals otherwise entirely different, but living within the same geographical area, ... the connection by series of special structures observed in animals widely scattered over the surface of the globe, ... the definite relations in which animals stand to the surrounding world, ... the relations in which individuals of the same species stand to one another, ... the limitation of the range of changes which animals undergo during their growth, ... the return to a definite norm of animals which multiply in various ways, ... the order of succession of the different types of animals and plants characteristic of the

* Agassiz, Essay on Classification; Contrib. to Nat. Hist., i, p. 132, et seq.

different geological epochs, ... the localization of some types of animals upon the same points of the surface of the globe during several successive geological periods; .... the parallelism between the order of succession of animals and plants in geological times, and the gradation among their living representatives, ... the parallelism between the order of succession of animals in geological times and the changes their living representatives undergo during their embryological growth,* .... *the combination in many extinct types of characters which in later ages appear disconnected in different types*, ... the parallelism between the gradation among animals and the changes they undergo during their growth, ... the relations existing between these different series and the geographical distribution of animals, ... the connection of all the known features of nature into one system,—"

In a word, the whole relations of animals, &c. to surrounding nature and to each other, are regarded under the one view as ultimate facts, or in their ultimate aspect, and interpreted theologically;—under the other as complex facts, to be analyzed and interpreted scientifically. The one naturalist, perhaps too largely assuming the scientifically unexplained to be inexplicable, views the phenomena only in their supposed relation to the Divine mind. The other, naturally expecting many of these phenomena to be resolvable under investigation, views them in their relations to one another, and endeavors to explain them as far as he can (and perhaps farther) through natural causes.

But does the one really exclude the other? Does the investigation of physical causes stand opposed to the theological view and the study of the harmonies between mind and Nature? More than this, is it not most presumable that an intellectual conception realized in nature would be realized through natural agencies? Mr. Agassiz answers these questions affirmatively when he declares that "the task of science is to investigate what has been done, to enquire if possible *how it has been done*, rather than to ask what is possible for the Deity, since *we can know that only by what actually exists*;" and also when he extends the argument for the intervention in nature of a creative mind to its legitimate application in the inorganic world; which, he remarks, "considered in the same light, would not fail also to exhibit unexpected evidence of thought, in the character of the laws regulating the chemical combinations, the action of physi-

* As to this, Darwin remarks that he can only hope to see the law hereafter proved true (p. 449); and p. 338: "Agassiz insists that ancient animals resemble to a certain extent the embryos of recent animals of the same classes; or that the geological succession of extinct forms is in some degree parallel to the embryological development of recent forms. I must follow Pictet and Huxley in thinking that the truth of this doctrine is very far from proved. Yet I fully expect to see it hereafter confirmed, at least in regard to subordinate groups, which have branched off from each other within comparatively recent times. For this doctrine of Agassiz accords well with the theory of natural selection."

cal forces, etc., etc."* Mr. Agassiz, however, pronounces that "the connection between the facts is *only intellectual*;"—an opinion which the analogy of the inorganic world, just referred to, does not confirm, for there a material connection between the facts is justly held to be consistent with an intellectual,—and which the most analogous cases we can think of in the organic world do not favor; for there is a material connection between the grub, the pupa, and the butterfly, between the tadpole and the frog, or, still better, between those distinct animals which succeed each other in alternate and very dissimilar generations. So that mere analogy might rather suggest a natural connection than the contrary; and the contrary cannot be demonstrated until the possibilities of nature under the Deity are fathomed.

But the intellectual connection being undoubted, Mr. Agassiz properly refers the whole to "the agency of Intellect as its first cause." In doing so, however, he is not supposed to be offering a scientific explanation of the phenomena. Evidently he is considering only the ultimate *why*, not the proximate why or *how*.

Now the latter is just what Mr. Darwin is considering. He conceives of a physical connection between allied species: but we suppose he does not deny their intellectual connection, as related to a Supreme Intelligence. Certainly we see no reason why he should, and many reasons why he should not. Indeed, as we contemplate the actual direction of investigation and speculation in the physical and natural sciences, we dimly apprehend a probable synthesis of these divergent theories, and in it the ground for a strong stand against mere naturalism. Even if the doctrine of the origin of species through natural selection should prevail in our day, we shall not despair; being confident that the genius of an Agassiz will be found equal to the work of constructing, upon the mental and material foundations combined, a theory of nature as theistic and as scientific, as that which he has so eloquently expounded.

To conceive the possibility of "the descent of species from species by insensibly fine gradations" during a long course of time, and to demonstrate its compatibility with a strictly theistic view of the universe, is one thing: to substantiate the theory itself or show its likelihood is quite another thing. This brings us to consider what Darwin's theory actually is, and how he supports it.

That the existing kinds of animals and plants, or many of them, may be derived from other and earlier kinds, in the lapse

* Op. cit., p. 131.—One or two Bridgewater Treatises, and most modern works upon Natural Theology should have rendered the evidences of thought in inorganic nature not "unexpected."

of time, is by no means a novel proposition. Not to speak of ancient speculations of the sort, it is the well-known Lamarckian theory. The first difficulty which such theories meet with is that, in the present age, with all its own and its inherited prejudgments, the whole burden of proof is naturally, and indeed properly, laid upon the shoulders of the propounders; and thus far the burden has been more than they could bear. From the very nature of the case, substantive proof of specific creation is not attainable; but that of derivation or transmutation of species may be. He who affirms the latter view is bound to do one or both of two things. Either, 1, to assign real and adequate causes, the natural or necessary result of which must be to produce the present diversity of species and their actual relations; or, 2, to show the general conformity of the whole body of facts to such assumption, and also to adduce instances explicable by it and inexplicable by the received view,—so perhaps winning our assent to the doctrine, through its competency to harmonize all the facts, even though the cause of the assumed variation remain as occult as that of the transformation of tadpoles into frogs, or that of *Coryne* into *Sarzia*.

The first line of proof, successfully carried out, would establish derivation as a true physical theory; the second, as a sufficient hypothesis.

Lamarck mainly undertook the first line, in a theory which has been so assailed by ridicule that it rarely receives the credit for ability to which in its day it was entitled. But he assigned partly unreal, partly insufficient causes; and the attempt to account for a progressive change in species through the direct influence of physical agencies, and through the appetencies and habits of animals reacting upon their structure, thus causing the production and the successive modification of organs, is a conceded and total failure. The shadowy author of the Vestiges of the Natural History of Creation can hardly be said to have undertaken either line, in a scientific way. He would explain the whole progressive evolution of nature by virtue of an inherent tendency to development,—thus giving us an idea or a word in place of a natural cause, a restatement of the proposition instead of an explanation. Mr. Darwin attempts both lines of proof, and in a strictly scientific spirit; but the stress falls mainly upon the first; for, as he does assign real causes, he is bound to prove their adequacy.

It should be kept in mind that, while all direct proof of independent origination is unattainable from the nature of the case, the overthrow of particular schemes of derivation has not established the opposite proposition. The futility of each hypothesis thus far proposed to account for derivation may be made apparent, or unanswerable objections may be urged against

it; and each victory of the kind may render derivation more improbable, and therefore specific creation more probable, without settling the question either way. New facts, or new arguments and a new mode of viewing the question may some day change the whole aspect of the case. It is with the latter that Mr. Darwin now reopens the discussion.

Having conceived the idea that varieties are incipient species, he is led to study variation in the field where it shows itself most strikingly and affords the greatest facilities to investigation. Thoughtful naturalists have had increasing grounds to suspect that a re-examination of the question of species in zoology and botany, commencing with those races which man knows most about, viz. the domesticated and cultivated races, would be likely somewhat to modify the received idea of the entire fixity of species. This field, rich with various but unsystematized stores of knowledge accumulated by cultivators and breeders, has been generally neglected by naturalists, because these races are not in a state of nature; whereas they deserve particular attention on this very account, as experiments, or the materials for experiments, ready to our hand. In domestication we vary some of the natural conditions of a species, and thus learn experimentally what changes are within the reach of varying conditions in nature. We separate and protect a favorite race against its foes or its competitors, and thus learn what it might become if nature ever afforded it equal opportunities. Even when, to subserve human uses, we modify a domesticated race to the detriment of its native vigor, or to the extent of practical monstrosity, although we secure forms which would not be originated and could not be perpetuated in free nature, yet we attain wider and juster views of the possible degree of variation. We perceive that some species are more variable than others, but that no species subjected to the experiment persistently refuses to vary; and that when it has once begun to vary, its varieties are not the less but the more subject to variation. "No case is on record of a variable being ceasing to be variable under cultivation." It is fair to conclude, from the observation of plants and animals in a wild as well as domesticated state, that the tendency to vary is general, and even universal. Mr. Darwin does "not believe that variability is an inherent and necessary contingency, under all circumstances, with all organic beings, as some authors have thought." No one supposes variation could occur under all circumstances; but the facts on the whole imply an universal tendency, ready to be manifested under favorable circumstances. In reply to the assumption that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates, it is asked:

"How could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass or guinea-fowl, or the small power of endurance of warmth by the rein-deer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would vary on an average as largely as the parent species of our existing domesticated productions have varied."

As to amount of variation, there is the common remark of naturalists that the varieties of domesticated plants or animals often differ more widely than do the individuals of distinct species in a wild state: and even in nature the individuals of some species are known to vary to a degree sensibly wider than that which separates related species. In his instructive section on the breeds of the domestic pigeon, our author remarks that:—"at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in the same genus; more especially as in each of these breeds several truly inherited sub-breeds, or species as he might have called them, could be shown him." That this is not a case like that of dogs, in which probably the blood of more than one species is mingled, Mr. Darwin proceeds to show, adducing cogent reasons for the common opinion that all have descended from the wild rock-pigeon. Then follow some suggestive remarks:—

"I have discussed the probable origin of domestic pigeons at some, yet quite insufficient, length; because when I first kept pigeons and watched the several kinds, knowing well how true they bred, I felt fully as much difficulty in believing that they could ever have descended from a common parent, as any naturalist could in coming to a similar conclusion in regard to many species of finches, or other large groups of birds, in nature. One circumstance has struck me much; namely, that all the breeders of the various domestic animals and the cultivators of plants, with whom I have ever conversed, or whose treatises I have read, are firmly convinced that the several breeds to which each has attended, are descended from so many aboriginally distinct species. Ask, as I have asked, a celebrated raiser of Hereford cattle, whether his cattle might not have descended from long-horns, and he will laugh you to scorn. I have never met a pigeon, or poultry, or duck, or rabbit fancier, who was not fully convinced that each main breed was descended from a distinct species. Van Mons, in his treatise on pears and apples, shows how utterly he disbelieves that the several sorts, for instance a Ribston-pippin

or Codlin-apple, could ever have proceeded from the seeds of the same tree. Innumerable other examples could be given. The explanation, I think; is simple: from long-continued study they are strongly impressed with the differences between the several races; and though they well know that each race varies slightly, for they win their prizes by selecting such slight differences, yet they ignore all general arguments, and refuse to sum up in their minds slight differences accumulated during many successive generations. May not those naturalists who, knowing far less of the laws of inheritance than does the breeder, and knowing no more than he does of the intermediate links in the long lines of descent, yet admit that many of our domestic races have descended from the same parents—may they not learn a lesson of caution, when they deride the idea of species in a state of nature being lineal descendants of other species?"

The actual causes of variation are unknown. Mr. Darwin favors the opinion of the late Mr. Knight, the great philosopher of horticulture, that variability under domestication is somehow connected with excess of food. He also regards the unknown cause as acting chiefly upon the reproductive system of the parents, which system, judging from the effect of confinement or cultivation upon its functions, he concludes to be more susceptible than any other to the action of changed conditions of life. The tendency to vary certainly appears to be much stronger under domestication than in free nature. But we are not sure that the greater variableness of cultivated races is not mainly owing to the far greater opportunities for manifestation and accumulation—a view seemingly all the more favorable to Mr. Darwin's theory. The actual amount of certain changes, such as size or abundance of fruit, size of udder, stands of course in obvious relation to supply of food.

Really, we no more know the reason why the progeny occasionally deviates from the parent than we do why it usually resembles it. Though the laws and conditions governing variation are known to a certain extent, while those governing inheritance are apparently inscrutable. "Perhaps," Darwin remarks, "the correct way of viewing the whole subject would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly." This, from general and obvious considerations, we have long been accustomed to do. Now, as exceptional instances are expected to be capable of explanation, while ultimate laws are not, it is quite possible that variation may be accounted for, while the great primary law of inheritance remains a mysterious fact.

The common proposition is, that *species reproduce their like;* this is a sort of general inference, only a degree closer to fact than the statement that genera reproduce their like. The true proposition, the fact incapable of further analysis is, that *individ-*

uals reproduce their like,—that characteristics are inheritable. So varieties, or deviations once originated, are perpetuable, like species. Not so likely to be perpetuated, at the outset; for the new form tends to resemble a grand-parent and a long line of similar ancestors, as well as to resemble its immmediate progenitors. Two forces which coincide in the ordinary case, where the offspring resembles its parent, act in different directions when it does not, and it is uncertain which will prevail. If the remoter, but very potent ancestral influence predominates, the variation disappears with the life of the individual. If that of the immediate parent—feebler no doubt, but closer—the variety survives in the offspring; whose progeny now has a redoubled tendency to produce its own like; whose progeny again is almost sure to produce its like, since it is much the same whether it takes after its mother or its grandmother.

In this way races arise, which under favorable conditions may be as hereditary as species. In following these indications, watching opportunities, and breeding only from those individuals which vary most in a desirable direction, man leads the course of variation as he leads a streamlet,—apparently at will, but never against the force of gravitation,—to a long distance from its source, and makes it more subservient to his use or fancy. He unconsciously strengthens those variations which he prizes when he plants the seed of a favorite fruit, preserves a favorite domestic animal, drowns the uglier kittens of a litter, and allows only the handsomest or the best mousers to propagate. Still more, by methodical selection, in recent times almost marvellous results have been produced in new breeds of cattle, sheep, and poultry, and new varieties of fruit of greater and greater size or excellence.

It is said that all domestic varieties if left to run wild, would revert to their aboriginal stocks. Probably they would wherever various races of one species were left to commingle. At least the abnormal or exaggerated characteristics induced by high feeding, or high cultivation, and prolonged close breeding would promptly disappear, and the surviving stock would soon blend into a homogeneous result (in a way presently explained), which would naturally be taken for the original form; but we could seldom know if it were so. It is by no means certain that the result would be the same if the races ran wild each in a separate region. Dr. Hooker doubts if there is a true reversion in the case of plants. Mr. Darwin's observations rather favor it in the animal kingdom. With mingled races reversion seems well made out in the case of pigeons. The common opinion upon this subject therefore probably has some foundation. But even if we regard varieties as oscillations around a primitive centre or

type, still it appears from the readiness with which such varieties originate, that a certain amount of disturbance would carry them beyond the influence of the primordial attraction, where they may become new centres of variation.

Some suppose that races cannot be perpetuated indefinitely even by keeping up the conditions under which they were fixed: but the high antiquity of several, and the actual fixity of many of them, negative this assumption. "To assert that we could not breed our cart and race horses, long and short-horned cattle, and poultry of various breeds, for almost an infinite number of generations would be opposed to all experience."

Why varieties develope so readily and deviate so widely under domestication, while they are apparently so rare or so transient in free nature, may easily be shown. In nature, even with hermaphrodite plants, there is a vast amount of cross fertilization among various individuals of the same species. The inevitable result of this (as was long ago explained in this Journal*) is to repress variation, to keep the mass of a species comparatively homogeneous over any area in which it abounds in individuals. Starting from a suggestion of the late Mr. Knight, now so familiar, that close interbreeding diminishes vigor and fertility†; and perceiving that bisexuality is ever aimed at in nature,—being attained physiologically in numerous cases where it is not structurally,—Mr. Darwin has worked out the subject in detail, and shown how general is the concurrence, either habitual or occasional, of two hermaphrodite individuals in the reproduction of their kind; and has drawn the philosophical inference that probably no organic being self-fertilizes indefinitely; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable. We refer the reader to the section on the intercrossing of individuals (p. 96–101), and also to an article in the Gardeners' Chronicle a year and a half ago, for the details of a very interesting contribution to science, irrespective of theory.

In domestication, this intercrossing may be prevented; and in this prevention lies the art of producing varieties. But "the art itself is Nature," since the whole art consists in allowing the most universal of all natural tendencies in organic things (inheritance) to operate uncontrolled by other and obviously incidental tendencies. No new power, no artificial force is brought into play either by separating the stock of a desirable variety so as to prevent mixture, or by selecting for breeders those indi-

* Vol. xvii, [2], 1854, p. 13.

† We suspect that this is not an ultimate fact, but a natural consequence of inheritance,—the inheritance of disease or of tendency to disease, which close interbreeding perpetuates and accumulates, but wide breeding may neutralize or eliminate.

viduals which most largely partake of the peculiarities for which the breed is valued.*

We see everywhere around us the remarkable results which Nature may be said to have brought about under artificial selection and separation. Could she accomplish similar results when left to herself? Variations might begin, we know they do begin, in a wild state. But would any of them be preserved and carried to an equal degree of deviation? Is there anything in nature which in the long run may answer to artificial selection? Mr. Darwin thinks that there is; and *Natural Selection* is the key-note of his discourse.

As a preliminary, he has a short chapter to show that there is variation in nature, and therefore something for natural selection to act upon. He readily shows that such mere variations as may be directly referred to physical conditions (like the depauperation of plants in a sterile soil, or their dwarfing as they approach an alpine summit, the thicker fur of an animal from far northward, &c.), and also those individual differences which we everywhere recognize but do not pretend to account for, are not separable by any assignable line from more strongly marked varieties; likewise that there is no clear demarcation between the latter and subspecies, or varieties of the highest grade (distinguished from species not by any known inconstancy, but by the supposed lower importance of their characteristics); nor between these and recognized species. "These differences blend into each other in an insensible series, and the series impresses the mind with an idea of an actual passage."

This gradation from species downward is well made out. To carry it one step farther upwards, our author presents in a strong light the differences which prevail among naturalists as to what forms should be admitted to the rank of species. Some genera (and these in some countries) give rise to far more discrepancy than others; and it is concluded that the large or dominant genera are usually the most variable. In a flora so small as the British, 182 plants generally reckoned as varieties, have been ranked by some botanists as species. Selecting the British genera which include the most polymorphous forms, it appears that Babington's Flora gives them 251 species, Bentham's only 112, a difference of 139 doubtful forms. These are nearly the extreme views; but they are the views of two most capable and most experienced judges, in respect to one of the best known floras of the world. The fact is suggestive, that the best known countries furnish the greatest number of such doubtful cases.

* The rules and processes of breeders of animals, and their results, are so familiar that they need not be particularized. Less is popularly known about the production of vegetable races. We refer our readers back to this Journal, xxvii, pp. 440–442 (May, 1859) for an abstract of the papers of M. Vilmorin upon this subject.

Illustrations of this kind may be multiplied to a great extent. They make it plain that, whether species in nature are aboriginal and definite or not, our practical conclusions about them, as embodied in systematic works, are not *facts* but *judgments*, and largely fallible judgments.

How much of the actual coincidence of authorities is owing to imperfect or restricted observation, and to one naturalist's adopting the conclusions of another without independent observation, this is not the place to consider. It is our impression that species of animals are more definitely marked than those of plants; this may arise from our somewhat extended acquaintance with the latter, and our ignorance of the former. But we are constrained by our experience to admit the strong likelihood, in botany, that varieties on the one hand and what are called closely related species on the other do not differ except in degree. Whenever the wider difference separating the latter can be spanned by intermediate forms, as it sometimes is, no botanist long resists the inevitable conclusion. Whenever, therefore, this wider difference can be shown to be compatible with community of origin, and explained through natural selection or in any other way, we are ready to adopt the *probable* conclusion; and we see beforehand how strikingly the actual geographical association of related species favors the broader view. Whether we should continue to regard the forms in question as distinct species, depends upon what meaning we shall finally attach to that term; and that depends upon how far the doctrine of derivation can be carried back and how well it can be supported.

In applying his principle of natural selection to the work in hand, Mr. Darwin assumes, as we have seen: 1, some variability of animals and plants in nature; 2, the absence of any definite distinction between slight variations, and varieties of the highest grade; 3, the fact that naturalists do not practically agree, and do not increasingly tend to agree, as to what forms are species and what are strong varieties, thus rendering it probable that there may be no essential and original difference, or no possibility of ascertaining it, at least in many cases; also, 4, that the most flourishing and dominant species of the larger genera on an average vary most (a proposition which can be substantiated only by extensive comparisons, the details of which are not given);—and, 5, that in large genera the species are apt to be closely but unequally allied together, forming little clusters round certain species,—just such clusters as would be formed if we suppose their members once to have been satellites or varieties of a central or parent species, but to have attained at length a wider divergence and a specific character. The fact of such association is undeniable; and the use which Mr. Darwin makes of it seems fair and natural.

The gist of Mr. Darwin's work is to show that such varieties are gradually diverged into species and genera through *natural selection;* that natural selection is the inevitable result of the *struggle for existence* which all living things are engaged in; and that this struggle is an unavoidable consequence of several natural causes, but mainly of the high rate at which all organic beings tend to increase.

Curiously enough, Mr. Darwin's theory is grounded upon the doctrine of Malthus and the doctrine of Hobbes. The elder DeCandolle had conceived the idea of the struggle for existence, and in a passage which would have delighted the cynical philosopher of Malmesbury, had declared that all nature is at war, one organism with another or with external nature; and Lyell and Herbert had made considerable use of it. But Hobbes in his theory of society and Darwin in his theory of natural history, alone have built their systems upon it. However moralists and political economists may regard these doctrines in their original application to human society and the relation of population to subsistence, their thorough applicability to the great society of the organic world in general is now undeniable. And to Mr. Darwin belongs the credit of making this extended application, and of working out the immensely diversified results with rare sagacity and untiring patience. He has brought to view *real causes* which have been largely operative in the establishment of the actual association and geographical distribution of plants and animals. In this he must be allowed to have made a very important contribution to an interesting department of science, even if his theory fails in the endeavor to explain the origin or diversity of species.

"Nothing is easier," says our author, "than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, I am convinced that the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood. We behold the face of nature bright with gladness, we often see superabundance of food; we do not see, or we forget, that the birds which are idly singing round us mostly live on insects or seeds, and are thus constantly destroying life; or we forget how largely these songsters, or their eggs, or their nestlings, are destroyed by birds and beasts of prey; we do not always bear in mind, that though food may be now superabundant, it is not so at all seasons of each recurring year."—p. 62.

"There is no exception to the rule that every organic being naturally increases at so high a rate, that if not destroyed, the earth would soon be covered by the progeny of a single pair. Even slow-breeding man has doubled in twenty-five years, and at this rate, in a few thousand years, there would literally not be standing room for his progeny. Linnæus has calculated that if an annual plant produced only two seeds—

and there is no plant so unproductive as this—and their seedlings next year produced two, and so on, then in twenty years there would be a million plants. The elephant is reckoned to be the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase: it will be under the mark to assume that it breeds when thirty years old, and goes on breeding till ninety years old, bringing forth three pairs of young in this interval; if this be so, at the end of the fifth century there would be alive fifteen million elephants, descended from the first pair.

"But we have better evidence on this subject than mere theoretical calculations, namely, the numerous recorded cases of the astonishingly rapid increase of various animals in a state of nature, when circumstances have been favorable to them during two or three following seasons. Still more striking is the evidence from our domestic animals of many kinds which have run wild in several parts of the world; if the statements of the rate of increase of slow-breeding cattle and horses in South America, and latterly in Australia, had not been well authenticated, they would have been quite incredible. So it is with plants: cases could be given of introduced plants which have become common throughout whole islands in a period of less than ten years. Several of the plants now most numerous over the wide plains of La Plata, clothing square leagues of surface almost to the exclusion of all other plants, have been introduced from Europe; and there are plants which now range in India, as I hear from Dr. Falconer, from Cape Comorin to the Himalaya, which have been imported from America since its discovery. In such cases, and endless instances could be given, no one supposes that the fertility of these animals or plants has been suddenly and temporarily increased in any sensible degree. The obvious explanation is that the conditions of life have been very favorable, and that there has consequently been less destruction of the old and young, and that nearly all the young have been enabled to breed. In such cases the geometrical ratio of increase, the result of which never fails to be surprising, simply explains the extraordinarily rapid increase and wide diffusion of naturalized productions in their new homes."—pp. 64, 65.

"All plants and animals are tending to increase at a geometrical ratio; all would most rapidly stock any station in which they could anyhow exist; the increase must be checked by destruction at some period of life."—p. 65.

The difference between the most and the least prolific species is of no account.

"The condor lays a couple of eggs, and the ostrich a score; and yet in the same country the condor may be the more numerous of the two. The Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world."—p. 68.

"The amount of food gives the extreme limit to which each species can increase; but very frequently it is not the obtaining of food, but the serving as prey to other animals, which determines the average numbers of a species."—p. 68.

"Climate plays an important part in determining the average numbers of a species, and periodical seasons of extreme cold or drought, I believe

to be the most effective of all checks. I estimated that the winter of 1854–55 destroyed four-fifths of the birds in my own grounds; and this is a tremendous destruction, when we remember that ten per cent is an extraordinarily severe mortality from epidemics with man. The action of climate seems at first sight to be quite independent of the struggle for existence; but in so far as climate chiefly acts in reducing food, it brings on the most severe struggle between the individuals, whether of the same or of distinct species, which subsist on the same kind of food. Even when climate, for instance extreme cold, acts directly, it will be the least vigorous, or those which have got least food through the advancing winter, which will suffer most. When we travel from south to north, or from a damp region to a dry, we invariably see some species gradually getting rarer and rarer, and finally disappearing; and the change of climate being conspicuous, we are tempted to attribute the whole effect to its direct action. But this is a very false view: we forget that each species, even where it most abounds, is constantly suffering enormous destruction at some period of its life, from enemies or from competitors for the same place and food; and if these enemies or competitors be in the least degree favored by any slight change of climate, they will increase in numbers, and, as each area is already stocked with inhabitants, the other species will decrease. When we travel southward and see a species decreasing in numbers, we may feel sure that the cause lies quite as much in other species being favored, as in this one being hurt. So it is when we travel northward, but in a somewhat lesser degree, for the number of species of all kinds, and therefore of competitors, decreases northwards; hence in going northward, or in ascending a mountain, we far oftener meet with stunted forms, due to the *directly* injurious action of climate, than we do in proceeding southwards or in descending a mountain. When we reach the Arctic regions, or snow-capped summits, or absolute deserts, the struggle for life is almost exclusively with the elements.

"That climate acts in main part indirectly by favoring other species, we may clearly see in the prodigious number of plants in our gardens which can perfectly well endure our climate, but which never become naturalized, for they cannot compete with our native plants, nor resist destruction by our native animals."—pp. 68, 69.

After an instructive instance in which "cattle absolutely determine the existence of the Scotch Fir," we are referred to cases in which insects determine the existence of cattle.

"Perhaps Paraguay offers the most curious instance of this; for here neither cattle nor horses nor dogs have ever run wild, though they swarm southward and northward in a feral state; and Azara and Rengger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by birds. Hence, if certain insectivorous birds (whose numbers are probably regulated by hawks or beasts of prey) were to increase in Paraguay, the flies would decrease—then cattle and horses would become feral, and this would certainly

greatly alter (as indeed I have observed in parts of South America) the vegetation: this again would largely affect the insects; and this, as we just have seen in Staffordshire, the insectivorous birds, and so onwards in ever-increasing circles of complexity. We began this series by insectivorous birds, and we had ended with them. Not that in nature the relations can ever be as simple as this. Battle within battle must ever be recurring with varying success; and yet in the long run the forces are so nicely balanced, that the face of nature remains uniform for long periods of time, though assuredly the merest trifle would often give the victory to one organic being over another. Nevertheless so profound is our ignorance, and so high our presumption, that we marvel when we hear of the extinction of an organic being; and as we do not see the cause, we invoke cataclysms to desolate the world, or invent laws on the duration of the forms of life!"—pp. 72, 73.

"When we look at the plants and bushes clothing an entangled bank, we are tempted to attribute their proportional numbers and kinds to what we call chance. But how false a view is this! Every one has heard that when an American forest is cut down, a very different vegetation springs up; but it has been observed that the trees now growing on the ancient Indian mounds, in the Southern United States, display the same beautiful diversity and proportion of kinds as in the surrounding virgin forests. What a struggle between the several kinds of trees must here have gone on during long centuries, each annually scattering its seeds by the thousand; what war between insect and insect—between insects, snails, and other animals with birds and beasts of prey—all striving to increase, and all feeding on each other or on the trees or their seeds and seedlings, or on the other plants which first clothed the ground and thus checked the growth of the trees! Throw up a handful of feathers, and all must fall to the ground according to definite laws; but how simple is this problem compared to the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins!"—pp. 74, 75.

For reasons obvious upon reflection the competition is often, if not generally, most severe between nearly related species when they are in contact, so that one drives the other before it, as the Hanoverian the old English rat, the small Asiatic cockroach in Russia, its greater congener, &c.: and this, when duly considered, explains many curious results;—such, for instance, as the considerable number of different genera of plants and animals which are generally found to inhabit any limited area.

"The truth of the principle, that the greatest amount of life can be supported by great diversification of structure, is seen under many natural circumstances. In an extremely small area, especially if freely open to immigration, and where the contest between individual and individual must be severe, we always find great diversity in its inhabitants. For instance, I found that a piece of turf, three feet by four in size, which had been exposed for many years to exactly the same conditions, supported twenty species of plants, and these belonged to eighteen genera and to

eight orders, which showed how much these plants differed from each other. So it is with the plants and insects on small and uniform islets; and so in small ponds of fresh water. Farmers find that they can raise most food by a rotation of plants belonging to the most different orders; nature follows what may be called a simultaneous rotation. Most of the animals and plants which live close round any small piece of ground, could live on it (supposing it not to be in any way peculiar in its nature), and may be said to be striving to the utmost to live there; but, it is seen, that where they come into the closest competition with each other, the advantages of diversification of structure, with the accompanying differences of habit and constitution, determine that the inhabitants, which thus jostle each other most closely, shall as a general rule, belong to what we call different genera and orders."—p. 114.

The abundance of some forms, the rarity and final extinction of many others, and the consequent divergence of character or increase of difference among the surviving representatives are other consequences. As favored forms increase, the less favored must diminish in number, for there is not room for all; and the slightest advantage, at first probably inappreciable to human observation, must decide which shall prevail and which must perish, or be driven to another and for it more favorable locality.

We cannot do justice to the interesting chapter upon natural selection by separated extracts. The following must serve to show how the principle is supposed to work.

"If during the long course of ages and under varying conditions of life, organic beings vary at all in the several parts of their organization, and I think this cannot be disputed; if there be, owing to the high geometrical powers of increase of each species, at some age, season, or year, a severe struggle for life, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of existence, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, I think it would be a most extraordinary fact if no variation ever had occurred useful to each being's own welfare, in the same way as so many variations have occurred useful to man. But if variations useful to any organic being do occur, assuredly individuals thus characterized will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance they will tend to produce offspring similarly characterized. This principle of preservation, I have called, for the sake of brevity, Natural Selection."—pp. 126, 127.

"In order to make it clear how, as I believe, natural selection acts, I must beg permission to give one or two imaginary illustrations. Let us take the case of a wolf, which preys on various animals, securing some by craft, some by strength, and some by fleetness; and let us suppose that the fleetest prey, a deer for instance, had from any change in the country increased in numbers, or that other prey had decreased in numbers, during that season of the year when the wolf is hardest pressed for food. I can under such circumstances see no reason to doubt that the swiftest and slimmest wolves would have the best chance of surviving,

and so be preserved or selected,—provided always that they retained strength to master their prey at this or at some other period of the year, when they might be compelled to prey on other animals. I can see no more reason to doubt this, than that man can improve the fleetness of his greyhounds by careful and methodical selection, or by that unconscious selection which results from each man trying to keep the best dogs without any thought of modifying the breed.

"Even without any change in the proportional numbers of the animals on which our wolf preyed, a cub might be born with an innate tendency to pursue certain kinds of prey. Nor can this be thought very improbable; for we often observe great differences in the natural tendencies of our domestic animals; one cat, for instance, taking to catch rats, another mice; one cat, according to Mr. St.John, bringing home winged game, another hares or rabbits, and another hunting on marshy ground and almost nightly catching woodcocks or snipes. The tendency to catch rats rather than mice is known to be inherited. Now, if any slight innate change of habit or of structure benefited an individual wolf, it would have the best chance of surviving and of leaving offspring. Some of its young would probably inherit the same habits or structure, and by the repetition of this process, a new variety might be formed which would either supplant or coexist with the parent-form of wolf. Or, again, the wolves inhabiting a mountainous district, and those frequenting the lowlands, would naturally be forced to hunt different prey; and from the continued preservation of the individuals best fitted for the two sites, two varieties might slowly be formed. These varieties would cross and blend where they met; but to this subject of intercrossing we shall soon have to return. I may add, that, according to Mr. Pierce, there are two varieties of the wolf inhabiting the Catskill Mountains in the United States, one with a light greyhound-like form, which pursues deer, and the other more bulky, with shorter legs, which more frequently attacks the shepherd's flocks."—pp. 90, 91.

We eke out the illustration here with a counterpart instance, viz., the remark of Dr. Bachman that "The deer that reside permanently in the swamps of Carolina are taller and longer-legged than those in the higher grounds."*

The limits allotted to this article are nearly reached, yet only four of the fourteen chapters of the volume have been touched. These, however, contain the fundamental principles of the theory and most of those applications of it which are capable of something like verification, relating as they do to phenomena now occurring. Some of our extracts also show how these principles are thought to have operated through the long lapse of the ages. The chapters from the sixth to the ninth inclusive are designed to obviate difficulties and objections, "some of them so grave that to this day," the author frankly says, he "can never reflect on them without being staggered." We do not wonder at it. After drawing what comfort he can from "the imperfection of

* Quadrupeds of America, ii, p. 239.

the geological record" (chap. 9), which we suspect is scarcely exaggerated, the author considers the geological succession of organic beings (chap. 10), to see whether they better accord with the common view of the immutability of species, or with that of their slow and gradual modification. Geologists must settle that question. Then follow two most interesting and able chapters on the geographical distribution of plants and animals, the summary of which we should be glad to cite; then a fitting chapter upon classification, morphology, embryology, &c., as viewed in the light of this theory, closes the argument; the fourteenth chapter being a recapitulation.

The interest for the general reader heightens as the author advances on his perilous way and grapples manfully with the most formidable difficulties.

To account, upon these principles, for the gradual elimination and segregation of nearly allied forms,—such as varieties, subspecies, and closely related or representative species,—also in a general way for their geographical association and present range, is comparatively easy, is apparently within the bounds of possibility, and even of probability. Could we stop here we should be fairly contented. But, to complete the system, to carry out the principles to their ultimate conclusion, and to explain by them many facts in geographical distribution which would still remain anomalous, Mr. Darwin is equally bound to account for the formation of genera, families, orders, and even classes, by natural selection. He does "not doubt that the theory of descent with modification embraces all the members of the same class," and he concedes that analogy would press the conclusion still farther; while he admits that "the more distinct the forms are, the more the arguments fall away in force." To command assent we naturally require decreasing probability to be overbalanced by an increased weight of evidence. An opponent might plausibly, and perhaps quite fairly, urge that the links in the chain of argument are weakest just where the greatest stress falls upon them.

To which Mr. Darwin's answer is, that the best parts of the testimony have been lost. He is confident that intermediate forms must have existed; that in the olden times when the genera, the families and the orders diverged from their parent stocks, gradations existed as fine as those which now connect closely related species with varieties. But they have passed and left no sign. The geological record, even if all displayed to view, is a book from which not only many pages, but even whole alternate chapters have been lost out, or rather which were never printed from the autographs of nature. The record was actually made in fossil lithography only at certain times and under certain conditions (i. e., at periods of slow subsidence and places of

abundant sediment); and of these records all but the last volume is out of print; and of its pages only local glimpses have been obtained. Geologists, except Lyell, will object to this,—some of them moderately, others with vehemence. Mr. Darwin himself admits, with a candor rarely displayed on such occasions, that he should have expected more geological evidence of transition than he finds, and that all the most eminent palæontologists maintain the immutability of species.

The general fact, however, that the fossil fauna of each period as a whole is nearly intermediate in character between the preceding and the succeeding faunas, is much relied on. We are brought one step nearer to the desired inference by the similar "fact, insisted on by all palæontologists, that fossils from two consecutive formations are far more closely related to each other, than are the fossils of two remote formations. Pictet gives a well-known instance,—the general resemblance of the organic remains from the several stages of the chalk formation, though the species are distinct at each stage. This fact alone, from its generality seems to have shaken Professor Pictet in his firm belief in the immutability of species." (p. 335.) What Mr. Darwin now particularly wants to complete his inferential evidence is a proof that the same gradation may be traced in later periods, say in the tertiary, and between that period and the present; also that the later gradations are finer, so as to leave it doubtful whether the succession is one of species,—believed on the one theory to be independent, on the other, derivative,—or of varieties, which are confessedly derivative. The proof of the finer gradation appears to be forthcoming. Des Hayes and Lyell have concluded that many of the middle tertiary, and a large proportion of the later tertiary mollusca are specifically identical with living species; and this is still the almost universally prevalent view. But Mr. Agassiz states that, "in every instance where he had sufficient materials, he had found that the species of the two epochs supposed to be identical by Des Hayes and Lyell were in reality distinct, although closely allied species."* Moreover he is now satisfied, as we understand, that the same gradation is traceable not merely in each great division of the tertiary, but in particular deposits or successive beds, each answering to a great number of years; where what have passed unquestioned as members of one species, upon closer examination of numerous specimens exhibit differences which in his opinion entitle them to be distinguished into two, three, or more species. It is plain, therefore, that whatever conclusions can be fairly drawn from the present animal and vegetable kingdoms in favor of a gradation of varieties into species, or into what

* Proceedings of the American Academy of Arts and Sciences, iv, p. 178.

may be regarded as such, the same may be extended to the tertiary period. In both cases, what some call species others call varieties; and in the later tertiary shells this difference in judgment affects almost half of the species!

We pass to a second difficulty in the way of Mr. Darwin's theory; to a case where we are perhaps entitled to demand of him evidence of gradation like that which connects the present with the tertiary mollusca. Wide, very wide is the gap, anatomically and physiologically (we do not speak of the intellectual) between the highest quadrumana and man; and comparatively recent, if ever, must the line have bifurcated. But where is there the slightest evidence of a common progenitor? Perhaps Mr. Darwin would reply by another question: where are the fossil remains of the men who made the flint knives and arrow-heads of the Somme valley?

We have a third objection, one, fortunately, which has nothing to do with geology. We can only state it here, in brief terms. The chapter on hybridism is most ingenious, able, and instructive. If sterility of crosses is a special, original arrangement to prevent the confusion of species by mingling, as is generally assumed, then, since varieties cross readily and their offspring is fertile *inter se*, there is a fundamental distinction between varieties and species. Mr. Darwin therefore labors to show that it is not a special endowment, but an incidental acquirement. He does show that the sterility of crosses is of all degrees;—upon which we have only to say, *Natura non facit saltum*, here any more than elsewhere. But, upon his theory he is bound to show how sterility might be acquired, through natural selection or through something else. And the difficulty is, that, whereas individuals of the very same blood tend to be sterile, and somewhat remoter unions diminish this tendency, and when they have diverged into two varieties the cross-breeds between the two are more fertile than either pure stock,—yet when they have diverged only one degree more the whole tendency is reversed, and the mongrel is sterile, either absolutely or relatively. He who explains the genesis of species through purely natural agencies should assign a natural cause for this remarkable result; and this Mr. Darwin has not done. Whether original or derived, however, this arrangement to keep apart those forms which have, or have acquired (as the case may be) a certain moderate amount of difference, looks to us as much designed for the purpose, as does a ratchet to prevent reverse motion in a wheel. If species have originated by divergence, this keeps them apart.

Here let us suggest a possibly attainable test of the theory of derivation, a kind of instance which Mr. Darwin may be fairly asked to produce,—viz., an instance of two varieties, or what may

be assumed as such, which have diverged enough to reverse the movement, to bring out some sterility in the crosses. The best marked human races might offer the most likely case. If mulattoes are sterile or tend to sterility, as some naturalists confidently assert, they afford Mr. Darwin a case in point. If, as others think, no such tendency is made out, the required evidence is wanting.

A fourth and the most formidable difficulty is that of the production and specialization of organs.

It is well said that all organic beings have been formed on two great laws; Unity of type, and Adaptation to the conditions of existence.* The special teleologists, such as Paley, occupy themselves with the latter only; they refer particular facts to special design, but leave an overwhelming array of the widest facts inexplicable. The morphologists build on unity of type, or that fundamental agreement in the structure of each great class of beings, which is quite independent of their habits or conditions of life; which requires each individual "to go through a certain formality," and to accept, at least for a time, certain organs, whether they are of any use to him or not. Philosophical minds form various conceptions for harmonizing the two views theoretically. Mr. Darwin harmonizes and explains them naturally. Adaptation to the conditions of existence is the result of Natural Selection; Unity of type, of unity of descent. Accordingly, as he puts his theory, he is bound to account for the origination of new organs, and for their diversity in each great type, for their specialization, and every adaptation of organ to function and of structure to condition, through natural agencies. Whenever he attempts this he reminds us of Lamarck, and shows us how little light the science of a century devoted to structural investigation has thrown upon the mystery of organization. Here purely natural explanations fail. The organs being given, natural selection may account for some improvement; if given of a variety of sorts or grades, natural selection might determine which should survive and where it should prevail.

On all this ground the only line for the theory to take is to make the most of gradation and adherence to type as suggestive of derivation, and unaccountable upon any other scientific view, —deferring all attempts to explain *how* such a metamorphosis was effected, until naturalists have explained *how* the tadpole is metamorphosed into a frog, or one sort of polyp into another. As to *why* it is so, the philosophy of efficient cause, and even the whole argument from design, would stand, upon the admission of such a theory of derivation, precisely where they stand without it. At least there is, or need be, no ground of differ-

* Owen adds a third, viz:—Vegetative Repetition; but this, in the vegetable kingdom is simply Unity of Type.

ence here between Darwin and Agassiz. The latter will admit, with Owen and every morphologist, that hopeless is the attempt to explain the similarity of pattern in members of the same class by utility or the doctrine of final causes. "On the ordinary view of the independent creation of each being, we can only say that so it is, that it has so pleased the Creator to construct each animal and plant." Mr. Darwin, in proposing a theory which suggests a *how* that harmonizes these facts into a system, we trust implies that all was done wisely, in the largest sense designedly, and by an Intelligent First Cause. The contemplation of the subject on the intellectual side, the amplest exposition of the Unity of Plan in Creation, considered irrespective of natural agencies, leads to no other conclusion.

We are thus, at last, brought to the question; what would happen if the derivation of species were to be substantiated, either as a true physical theory, or as a sufficient hypothesis? What would come of it? The enquiry is a pertinent one, just now. For, of those who agree with us in thinking that Darwin has not established his theory of derivation, many will admit with us that he has rendered a theory of derivation much less improbable than before; that such a theory chimes in with the established doctrines of physical science, and is not unlikely to be largely accepted long before it can be proved. Moreover, the various notions that prevail,—equally among the most and the least religious,—as to the relations between natural agencies or phenomena and Efficient Cause, are seemingly more crude, obscure, and discordant than they need be.

It is not surprising that the doctrine of the book should be denounced as atheistical. What does surprise and concern us is, that it should be so denounced by a scientific man, on the broad assumption that a material connection between the members of a series of organized beings is inconsistent with the idea of their being intellectually connected with one another through the Deity, i. e., as products of one mind, as indicating and realizing a preconceived plan. An assumption the rebound of which is somewhat fearful to contemplate, but fortunately one which every natural birth protests against.

It would be more correct to say, that the theory in itself is perfectly compatible with an atheistic view of the universe. That is true; but it is equally true of physical theories generally. Indeed, it is more true of the theory of gravitation, and of the nebular hypothesis, than of the hypothesis in question. The latter merely takes up *a particular, proximate cause*, or set of such causes, from which, it is argued, the present diversity of species has or may have *contingently* resulted. The author does not say *necessarily* resulted; that the actual results in mode and measure, and none other must have taken place. On the other

hand the theory of gravitation, and its extension in the nebular hypothesis, assume a *universal and ultimate* physical cause, from which the effects in nature must *necessarily* have resulted. Now it is not thought, at least at the present day, that the establishment of the Newtonian theory was a step towards atheism or pantheism. Yet the great achievement of Newton consisted in proving that certain forces, (blind forces, so far as the theory is concerned,) acting upon matter in certain directions, must *necessarily* produce planetary orbits of the exact measure and form in which observation shows them to exist;—a view which is just as consistent with eternal necessity, either in the atheistic or the pantheistic form, as it is with theism.

Nor is the theory of derivation particularly exposed to the charge of the atheism of fortuity; since it undertakes to assign real causes for harmonious and systematic results. But of this, a word at the close.

The value of such objections to the theory of derivation may be tested by one or two analogous cases. The common scientific as well as popular belief is that of the original, independent creation of oxygen and hydrogen, iron, gold, and the like. Is the speculative opinion, now increasingly held, that some or all of the supposed elementary bodies are derivative or compound, developed from some preceding forms of matter, irreligious? Were the old alchemists atheists as well as dreamers in their attempts to transmute earth into gold? Or, to take an instance from force (power),—which stands one step nearer to efficient cause than form—was the attempt to prove that heat, light, electricity, magnetism, and even mechanical power are variations or transmutations of one force, atheistical in its tendency? The supposed establishment of this view is reckoned as one of the greatest scientific triumphs of this century.

Perhaps, however, the objection is brought, not so much against the speculation itself, as against the attempt to show how derivation might have been brought about. Then the same objection applies to a recent ingenious hypothesis made to account for the genesis of the chemical elements out of the etherial medium, and to explain their several atomic weights and some other characteristics by their successive complexity,—hydrogen consisting of so many atoms of etherial substance united in a particular order, and so on. The speculation interested the philosophers of the British Association, and was thought innocent, but unsupported by facts. Surely Mr. Darwin's theory is none the worse, morally, for having some foundation in fact.

In our opinion, then, it is far easier to vindicate a theistic character for the derivative theory, than to establish the theory itself upon adequate scientific evidence. Perhaps scarcely any philosophical objection can be urged against the former to which

the nebular hypothesis is not equally exposed. Yet the nebular hypothesis finds general scientific acceptance, and is adopted as the basis of an extended and recondite illustration in Mr. Agassiz's great work.*

How the author of this book harmonizes his scientific theory with his philosophy and theology, he has not informed us. Paley, in his celebrated analogy with the watch, insists that if the time-piece were so constructed as to produce other similar watches, after the manner of generation in animals, the argument from design would be all the stronger. What is to hinder Mr. Darwin from giving Paley's argument a further *a-fortiori* extension to the supposed case of a watch which sometimes produces better watches, and contrivances adapted to successive conditions, and so at length turns out a chronometer, a town-clock, or a series of organisms of the same type? From certain incidental expressions at the close of the volume, taken in connection with the motto adopted from Whewell, we judge it probable that our author regards the whole system of nature as one which had received at its first formation the impress of the will of its Author, foreseeing the varied yet necessary laws of its action throughout the whole of its existence, ordaining when and how each particular of the stupendous plan should be realized in effect, and—with Him to whom to will is to do—in ordaining doing it. Whether profoundly philosophical or not, a view maintained by eminent philosophical physicists and theologians, such as Babbage on the one hand and Jowett on the other, will hardly be denounced as atheism. Perhaps Mr. Darwin would prefer to express his idea in a more general way, by adopting the thoughtful words of one of the most eminent naturalists of this or any age, substituting the word *action* for 'thought,' since it is the former (from which alone the latter can be inferred) that he has been considering. "Taking nature as exhibiting thought for my guide, it appears to me that while human thought is consecutive, Divine thought is simultaneous, embracing at the same time and forever, in the past, the present and the future, the most diversified relations among hundreds of thousands of organized beings, each of which may present complications again, which, to study and understand even imperfectly,—as for instance man himself—mankind has already spent thousands of years."† In thus conceiving of the Divine Power in act as cöetaneous with Divine Thought, and of both as far as may be apart from the human element of time, our author may regard the intervention of the Creator either as, humanly speaking, *done from all time*, or else as *doing through all time*. In the ultimate analysis we suppose that every philosophical theist must adopt one or the other conception.

* Contrib. Nat. Hist. Amer., i, p. 127–131. † Op. cit., p. 130.

A perversion of the first view leads towards atheism, the notion of an eternal sequence of cause and effect, for which there is no first cause,—a view which few sane persons can long rest in. The danger which may threaten the second view is pantheism. We feel safe from either error, in our profound conviction that there is order in the universe; that order presupposes mind; design, will; and mind or will, personality. Thus guarded, we much prefer the second of the two conceptions of causation, as the more philosophical as well as Christian view,—a view which leaves us with the same difficulties and the same mysteries in Nature as in Providence, and no other. Natural law, upon this view, is the human conception of continued and orderly Divine action.

We do not suppose that less power, or other power, is required to sustain the universe and carry on its operations, than to bring it into being. So, while conceiving no improbability of "interventions of Creative mind in nature," if by such is meant the bringing to pass of new and fitting events at fitting times, we leave it for profounder minds to establish, if they can, a rational distinction in kind between His working in nature carrying on operations, and in initiating those operations.

We wished under the light of such views, to examine more critically the doctrine of this book, especially of some questionable parts;—for instance, its explanation of the natural development of organs, and its implication of a "necessary acquirement of mental power" in the ascending scale of gradation. But there is room only for the general declaration that we cannot think the Cosmos a series which began with chaos and ends with mind, or of which mind is a result: that if by the successive origination of species and organs through natural agencies, the author means a series of events which succeed each other irrespective of a continued directing intelligence,—events which mind does not order and shape to destined ends,—then he has not established that doctrine, nor advanced towards its establishment, but has accumulated improbabilities beyond all belief. Take the formation and the origination of the successive degrees of complexity of eyes as a specimen. The treatment of this subject (pp. 188, 189), upon one interpretation is open to all the objections referred to; but if, on the other hand, we may rightly compare the eye "to a telescope, perfected by the long continued efforts of the highest human intellects," we could carry out the analogy, and draw satisfactory illustrations and inferences from it. The essential, the directly intellectual thing is the making of the improvements in the telescope or the steam-engine. Whether the successive improvements, being small at each step, and consistent with the general type of the instrument, are applied to some of the individual machines, or entire new ma-

chines are constructed for each, is a minor matter. Though if machines could engender, the adaptive method would be most economical; and economy is said to be a paramount law in nature. The origination of the improvements, and the successive adaptations to meet new conditions or subserve other ends, are what answer to the supernatural, and therefore remain inexplicable. As to bringing them into use, though wisdom foresees the result, the circumstances and the natural competition will take care of that, in the long run. The old ones will go out of use fast enough, except where an old and simple machine remains still best adapted to a particular purpose or condition,—as, for instance, the old Newcomen engine for pumping out coal-pits. If there's a Divinity that shapes these ends, the whole is intelligible and reasonable; otherwise, not.

We regret that the necessity of discussing philosophical questions has prevented a fuller examination of the theory itself, and of the interesting scientific points which are brought to bear in its favor. One of its neatest points, certainly a very strong one for the local origination of species, and their gradual diffusion under natural agencies, we must reserve for some other convenient opportunity.

The work is a scientific one, rigidly restricted to its direct object; and by its science it must stand or fall. Its aim is, probably not to deny creative intervention in nature,—for the admission of the independent origination of certain types does away with all antecedent improbability of as much intervention as may be required,—but to maintain that Natural Selection in explaining the facts, explains also many classes of facts which thousand-fold repeated independent acts of creation do not explain, but leave more mysterious than ever. How far the author has succeeded, the scientific world will in due time be able to pronounce.

As these sheets are passing through the press a copy of the second edition has reached us. We notice with pleasure the insertion of an additional motto on the reverse of the title-page, directly claiming the theistic view which we have vindicated for the doctrine. Indeed these pertinent words of the eminently wise Bishop Butler, comprise, in their simplest expression, the whole substance of our latter pages:—

"The only distinct meaning of the word 'natural' is *stated, fixed*, or *settled;* since what is natural as much requires and presupposes an intelligent mind to render it so, i. e., to effect it continually or at stated times, as what is supernatural or miraculous does to effect it for once." A. G.

Art. XVI.—*Forces;* by Theodore Lyman.

The first article in this Journal for November last* brings to mind the singular part which "force" now plays in science. The theory set forth in that article may be stated as follows: the world, and everything on it, may be considered as *matter;* this matter is not the same throughout, but consists of a certain number of ultimate species called *elements;* these elements are not always isolated, but are found joined to form, 1st, simple compounds, known sometimes as *minerals;* 2d, compounds of a nature higher, more complicated, and differently characterized, known as *vegetables;* 3d, compounds still higher and more complicated, and again differently characterized, known as *animals.* As the elements do not remain isolated, so also their compounds continually change their mutual relations; and the result of these changes is that continual falling down and building up which may be seen in the material world. To move these elements and their compounds there is a fund of *force,* constant in quantity and in quality; if ever it seems to be less in quantity, some of it is latent; if ever it seems different in quality, it is but changed in appearance, from being connected with some peculiar compound. Here is Cosmos at a glance!—there is the force, the mover, a; and these are the elements, the things moved, b, c, d, e, &c.—a may be a' (mechanical force), or a'' (chemical force), or a''' (vegetable force), &c., but still it remains a. a may act on b, c, d, e, and there may result such compounds as *be*, *ceb*, *dec*, &c. When a joins b to c, a part of a becomes *latent*, and the result may be called $bc+a$; but, when this compound is decomposed by a *different form* of a (e. g. a'''' or light) then a latent is *set free*, and immediately takes b and joins it to d, e, making the *higher compound bde*, while c is set free as an element. To give an instance, if b is carbon, c hydrogen, and d oxygen, and a''' is vegetable force, then b, c, and d, joined by the action of a''', would be the compound *bcd*, and might be a turnip. This theory looks simple, but its very roundness is suspicious.

The human mind, craving something more than mere fact, has tried to get at the *reason.* The fact is the law, the reason is the cause. It is in the search for the latter that scientific men have fallen on that unfortunate word, that shadow of a shadow, that last resort of ignorance—*Force!* It is safe to say that no

* The instructive and ingenious essay, by Prof. Joseph LeConte, is quoted in no invidious spirit, but simply as a fair sample of a philosophy now very common. Prof. LeConte may contend that he uses the word "force" only as a convenient supposition, on which to build a theory; if this be the case, it should be remembered that a form of expression which is sure to mislead ninety-nine readers in every hundred, should not be used in scientific writing.

word in the English language has created so much ambiguity, so much meaningless discussion, and so much wrong-headed philosophy, as this ill begotten monosyllable. It is a gag for inquisitive people. What keeps the world from flying off in space? The force of attraction. What keeps it from rushing towards the sun? The centrifugal force. What makes oxyd of lead join acetic acid? The chemical force. What makes oxyd of lead *leave* acetic acid and go to sulphuric acid? The chemical force again. What makes a fish with a ventral fin? The vital force. What makes a fish *without* a ventral fin? The vital force again. Mark this! These forces are blind and they are the same always, yet they make different things, on the one hand, while on the other, they repeat the same thing over and over again. How is this? Can a round auger bore a square hole? Or, can it bore any hole at all without *intellect* to guide it? If several forces are not enough to account for the phenomena of the world, what can we expect of *one* force? Yet to this Prof. LeConte would reduce us. His "correlation of forces" is one force; and his "conservation of forces" is the amount of this force. The *amount of force*, acting on the *sum* of the *material elements*, produces all motion and all being. The force is spoken of as "latent," "set free," "liberated," and "developed," and as being "furnished" and "supplied," by decomposition.

Force is either something or nothing, if it be nothing, it is not worthy a place in science; if it be something it may be considered like any other fact of philosophy. What, then, is force and whence comes our idea of it? To answer these questions it is necessary to define, 1st, true causation; 2d, variable sequences; 3d, invariable sequences. The operations of my own will are the only instances of true-causation of which I am conscious. The will, itself immaterial, works either on the material body, or on the immaterial mind. When the body is free and healthy the will has direct control over the voluntary muscles, but none over the involuntary. On the other hand, when the mind is free and healthy, the will has direct control over the whole of it. Whenever I will to do an act, mental or physical, I am *conscious* of using power. *Causation, then, is the act of x, using power to produce y.*

We may, in the outer world, see one thing happen immediately after another; yet, in a little while, we may see the first thing again, but this time *not* followed by the second. In such a case we say that the fact, that one thing followed another, was an accident. For instance, twelve mines of powder may explode, one after another, but probably this will not happen so again. *A variable sequence, then, is the fact that y has followed x, but will not, of necessity, do so again.*

We may, in the outer world, see a particular thing, which, as far as our experience goes, is always followed by another par-

ticular thing; for example, the twelve mines above mentioned are fired by electricity, and, as far as we know, a certain amount of electric heat in contact with gunpowder is followed by an explosion. *An invariable sequence, then, is the fact, that y always follows x.* To sum up; a man, by conscious exercise of will, discharged the battery (*efficient causation*); the electric spark was followed by an explosion of gunpowder (*invariable sequence*); certain other mines happened to explode, after this one, in quick succession (*variable sequence*).

The admission of universal, invariable sequence (i. e., every x is invariably followed by its peculiar y) is the ground-principle of the theory of necessity, which leads directly to pantheistic atheism, with its idealistic and materialistic branches. The sole object of science thus becomes, to find out all the xs in the universe and all the ys, and to give to each x its proper y. All these, when tabulated so as to show their fixed relations to each other, make up the course of nature, and human knowledge has then done its utmost and has nothing left to work on. This course of nature, if admitted as entirely true, may still be looked at in two ways: 1st, it may be said that this course is such as it is by reason of an *essence* which exists in matter; or, 2d, it may be stated, that the course is such as it is, and that no reason therefor can be given, or ought to be given. Such views as these might be allowable if all our knowledge were got from observations on the *outer* world, and without reference to our own consciousness of the structure of our minds; for we could not prove the existence of *power*, in the phenomena of the outer world; and, if we suspected it, we might still set it down as an inherent property of matter. But the moment *consciousness of self* is brought into the question, a new element is introduced, namely, *intelligence*. Consciousness is fundamental, and cannot be denied; consciousness of thought, that is intelligence, cannot be denied; consciousness of *power* has been denied, and with what truth? I am fully conscious, that I turn my mind to a given thought, and I sharply distinguish this act of *causation* from any *sequence*; here then is the immaterial will exercising *power* over the immaterial mind. If there is no such thing as power, whence comes the word, and whence the general idea of it? Again, I will to move some part of my body, and do move it, and I compare this power with that used in mental action, and I find them the same. There is this difference between the cases: in the first instance I move what is strictly myself, the unit, the immaterial; in the second instance I move what is only a frame, over which I have limited control; the compound, the material. And observe this; there is a difference between merely *wishing* and the act of *exerting power consciously*. I may wish that my legs would, all at once, walk off with my body;

but, if this should take place, I should be conscious that I was not exerting the power. Again, if, at my mere wish, a great rock should come to me, I should be conscious that something else moved it, and not myself. And, if my power of will was so extended, that I could thereby move rocks, I should be just as conscious of that power as I now am of the power to move my own body. A wish, followed by the thing wished for, is a sequence; a command, followed by the thing commanded, is power or causation. It is therefore wrong to say, that such sequences, if observed many times, would give me the idea that *I* exercised the power. When ignorant people attribute true causation to material bodies, (e. g., in the case of electricity,) they do so because they have the consciousness of power in themselves, and they attribute the same property to a body, which has in itself no power, but which shows signs of being acted upon by a power. If now it be said that matter has a certain essence, property, or what not, which makes it to do certain things at certain times, what is this essence or property but *power?* From our very idea of the word, anything that produces motion in another thing, either has or uses power. Still, though there may be power, is it *intelligent?* or do there exist a number of *un*intelligent powers, which so limit the scope of each other's action as to make a balance in the course of nature? (e. g., the power of centrifugal force balanced by the power of gravitation; the power of oxygen, produced by plants, balanced by the carbon, produced by animals; the power of insects, balanced by the birds that feed on them; the power of fruits, balanced by the parasitic fungi that grow on them.) To such a query, science is able to make this answer: "The phenomena observed in the outer world (*non-ego*) correspond to the phenomena of self (*ego*), which are produced by *intellect.*"* This is a sound induction; if it be not good, then no induction is good; and, if induction cannot stand, science must fall. To sum up, if it be admitted that there is an essence, inherent in matter, which necessitates its actions, then, 1st, this essence is *power;* and 2d, this power is *intelligent;* for, to deny that the essence was power would be to stultify our understanding, and, to deny this power to be intelligent, would be to ignore induction and to destroy science. It may be well also to take notice of the fact, that most theories, savoring of materialism, speak of some ultimate essence (force) which is at the bottom of all motion and action, as if such essence were fundamental and satisfactory; but this, after all, does not help the theory, for, if this essence is true matter, it moves, first *itself,* and then other matter, and, if it is a property, then this property moves first *itself* and then matter.

* For a full consideration of intellect, as shown in nature, see Essay on Classification, by Louis Agassiz.

There still remains the second way of looking at the course of nature: it is such as it is, and no reason therefor can be given. What is the exact meaning of this statement? Simply that the upholder of the doctrine resolves, so far as concerns the gaining of truth, to put trust in the testimony of his eyes, ears, touch, and other physical senses, and in nothing else. He is a strict positivist; he says: "I see x, feel y, and hear z; to me, then, x, y, and z exist as things seen, felt and heard, respectively. To me there is no causation, for I do not directly perceive it. I believe in memory, it tells me what I have formerly seen; I believe in the axiom, *cogito ergo sum*, because the very denial of thought implies the action of thought. My duty is to clearly understand and to tabulate all the observations I have made; but there it stops; I may make many inductions and may use them for convenience, but am not sure of them. One induction tells me the sun will rise to-morrow: for my convenience I say it will; but of this I am not sure; perhaps it will *not*:—it is a thing of the future and the future I cannot see. I attempt to explain nothing:—*you* attempt to explain, and you come to no certain results:—I am more modest. By my individual observation I know certain things, and all other things are to me only as what may be. The earth may, or may not, turn again on its axis; causation may, or may not, exist; there may, or may not, be a God."

The holder of this theory, when compared with the believers in kindred opinions, is, in a certain sense, consistent; because he only makes statements, which are true as far as they go; and does not attempt explanations, which must in the end prove unsatisfactory. But his theory is incomplete and therefore one-sided and untrue. On the one hand, he acknowledges consciousness, and through consciousness believes in all the phenomena of the *outer* world (*non-ego*) as isolated facts; while, on the other, he ignores the properties of *self* (*ego*) which are also given by consciousness, and are as reliable as any of its data. Thus, he acknowledges the motions of his voluntary muscles, of which he is conscious, but refuses to acknowledge his own power to move them, or to leave them at rest, of which he is also conscious. We need go no further than the idea of *intelligent power*, to find the whole trouble in this, and in many other theories.*

What interest does a true conception of the ever-working Creative Intellect give to science! This correspondence of the human with the Divine mind! The astronomer works out, with pencil and paper, the possible answers to a certain problem of

* For able presentations of the doctrines of free-will and of necessity, consult the writings of Prof. Francis Bowen, and of J. S. Mill. The idea of causation has been carried to its last analysis by Sir William Hamilton.

motion; he looks at the heavens, and there sees these answers, illustrated in the orbits of celestial bodies. The zoölogist, marking the changes of the embryo, thinks of these changes as so many different animals; deep in the rocks he finds all stages of this embryo, each represented by a species, perfect in its kind!

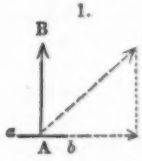
On the other hand, how dead the science, that puts "force" as its first cause! What is this force that makes the star-fish and the oyster, the medusa and the cuttle-fish, the crab and the whale, the tufted sea worm and the shark, each in its kind, and each telling its own story of manifold relations with animal creation, that is, that has been, and that is to come? Nature is no such simple thing that she should be dictated to by light, or heat, or electricity. These are her servants, not her masters!

Boston, Nov., 1859.

ART. XVII.—*On the causes of deviation in Elongated Projectiles*; by Maj. J. G. BARNARD, Corps of Engineers, U. S. A.

THE various and somewhat conflicting explanations given of the deviation of projectiles, both spherical and elongated, arising from their own rotary motions, leave room for a few additional words on this subject.

1.



If the plane surface $a\,b$ moves, in an elastic medium in the direction of its normal, with a velocity A B, that medium will oppose a force to which we apply the term "resistance" and which is measured by a function of the velocity A B.

If, at the same time, the surface has a velocity A C, *in its own plane*, the result will be an actual velocity of *each point of the surface*, represented by the diagonal A D; but the velocity of impact of the surface with the air, is the same in both cases, being due only to the normal velocity A B. The motion A C in its own plane, would displace, in no degree, the atmospheric particles, (except through the agency of that action known as friction—not now considered,) and would therefore generate no component of "resistance."

2.

If the sphere, whose great circle is A D E F, move through the air in the direction C B, with a velocity V, a resistance will be opposed to its motion which will, in magnitude, be a function of the diameter, and of the velocity V. If we leave out of consideration the force of *friction*, the character and intensity of the impact of the sphere with the

air will be identically the same, whether it possesses or not rotary motion: for in either case, the surface, considered *as a whole*, advances in identically the same manner—the displacement of atmospheric particles is the same, and the resulting resistance, the same.

Let the rotation be supposed about a horizontal axis, perpendicular to the line of flight, and in the direction A D. The velocity of the *individual points*, m, n—or if you choose—*elementary surface*, $m\,n$, will be the resultant of the rotary and translative velocities, and the little surface $m\,n$, instead of moving (at the instant) in the direction $n\,o$, will move in an oblique direction $n\,p$. But the *rotary* component of velocity lies in the plane of this elementary surface, and has, (as in the case of the *lateral* velocity A C of the plane, Fig. 1) no agency whatever in displacing the air, or in affecting the intensity or character of its impact.

These considerations will, perhaps, be rendered more clear by reflecting that the resistance of a fluid, is due and *due only* to the *displacement of its particles*—that when the centre of the sphere has advanced from C to B, the anterior surface has advanced from F A D to F′ A′ D′, and displaced the air in identically the same manner, whether the sphere revolves or not.

These considerations are so obvious that it seems superfluous to insist on them; yet few of the writers on this subject have exhibited a clear understanding of them; or rather it may be said that they exhibit the reverse.

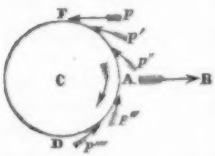
Thiroux, rejecting friction entirely, or rather considering its effects inappreciable, bases his reasoning on the higher velocity with which the points of the surface on the side A F *impinge on the air*, over that belonging to points on the side A D; an idea, as just shown, entirely fallacious. Capt. Neumann (Prussian artillery), in a theory as pretentious as it is unmeaning (Delobel's *Révue de Technologie Militaire*, vol. i.), carries this absurdity to the extreme of considering each elementary surface $m\,n$, *separately*, with its combined motion of translation and rotation, and, applying to each the ordinary expression for resistance of a plane surface impinging obliquely upon an elastic medium, *integrates* through each half of the anterior surface, to obtain the total action on each side.

Not only are the conventional expressions for the resistance of isolated oblique plane surfaces found most inaccurate in practice, but they lose *all* applicability when they cease to be *isolated*, and form part of another larger surface (not plane); and this problem, of which the knot is so expertly cut by Capt. Neumann, who proceeds to apply his results to the criticism or *test* of Magnus' and other theories, is the very "pièce de résistance" which has defied the analysis of d'Alembert, Poisson and Poncelet—per-

haps I might add of Newton and Laplace; one of those problems of mechanics to which the term *difficult* would be misapplied, for analysis has never yet been able to grasp it at all.

I have said that without the consideration of *friction*, the action upon the air of a rotating and non-rotating ball are identically the same. But *friction* materially alters the character of this action. Whatever may be the immediate cause of this force —whether simply a collision of the inequalities of the surface with the particles of the fluid—or whether it is due to *adhesion*, the effect is that the moving surface puts in motion *with it*, the adjacent fluid particles, and in so doing, developes forces tangential and opposed to its own motion.

3.

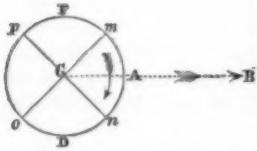


Thus the anterior surface of the sphere C, revolving from F to D, and advancing from A to B, creates, at each point, forces, p, p', p'' &c., tangential and opposed to its rotary motion, the resultant of which is a force acting from D towards F and tending to deflect the flight of the ball in that direction. This is the point of view, *and the sole one,* in which Poisson has considered the effects of friction.

But there is *another* effect which proves to be very powerful. Force cannot be applied to an elastic fluid, neither can motion be imparted or destroyed, without effecting, at the same time, its density and *pressure*. To retard a flowing current is to increase its pressure; to accelerate it is to diminish the same.

Applying this to the ball, the air, displaced and compressed in front, escapes along the surfaces A F and A D. Near its surface, the action of friction is to retard the escaping currents on the side A F, and to accelerate them on the side A D, and in consequence, an increase of pressure ensues on the side A F, and a diminution on the side A D; and therefore, a resulting pressure tending to deflect the ball from F towards D. If we divide the great circle A D F into four quadrants by the lines $m\,o$ and $n\,p$, drawn at angles of 45° with the direction of translation A B, we may better analyze the effects of friction, in the two forms in which I have presented them.

4.



The posterior quadrant $o\,p$ is in air so highly rarified that its action is insensible or nearly so.* On the side quadrant $m\,p$ the resultant of the forces of friction (the forces p, p', &c., of Fig. 3) are parallel (or nearly so) and *opposed* to the motion of translation. They have no effect (or but trifling) to *deflect* the ball from its course, but acting upon the air, in direct opposition to

* The *high velocities* of translation of military projectiles is supposed.

the escaping currents, their whole force is expended in destroying velocity and generating *pressure*. On the anterior quadrant *m n* the resultant of the forces *p*, *p'*, &c., is from *n* towards *m*, and tends, almost entirely, to deflect the ball in that direction.

On the quadrant *n o* the resultant of the forces *p*, *p'*, is parallel to the motion of translation, and co-incident in direction with the escaping current whose motion it accelerates and whose *pressure* it diminishes. Thus, taking the four quadrants, in *one*, *o p*, the forces of friction are absent; in *two*, *m p* and *n o*, they are expended in producing an *inequality of pressure* on the two sides of the ball, tending to deflect the ball towards the side D (right); while in the anterior quadrant *m n*, they act to deflect the ball in the opposite direction F (left).

It would have been difficult to decide *a priori*, which of these forces would prevail, though, while the force of friction is nugatory in *one* quadrant, in two (*m p* and *o n*) it expends itself in developing forces tending to deflect to the right, and in only *one*, *m n*, does its direct action tend to deflect to the left; yet it must be remarked that in this quadrant the air is most dense, friction the greatest, and that it acts *directly* upon the projectile.

In the two lateral quadrants the air is less dense, and it is only through pressures *developed in the air* that it produces its effect; a loss of effect ensuing in the medium through which it acts.

Experience has shown, however, that the forces developed in the two lateral quadrants prevail, and the projectile is deflected to the right; and the experiments of Dr. Magnus give the same result when, instead of a projectile moving through the air, a *current* of air is directed upon a revolving cylinder.*

The deviation of elongated projectiles, having rotary motion about their axis of figure, though many authors, Thiroux, Panôt, Tamissier, &c., have attempted to refer it to the same causes which produce the deviation in spherical balls, is evidently governed by other causes.

Not only do such writers have to make, as to the direction which the axis maintains, assumptions which conflict with each

* Of course the division into quadrants which I have made is arbitrary, and only used as a simple means of illustrating how the conflicting effects are produced from one and the same *cause*. I comprehend under the term *friction*, all the forces by which a solid *surface* acts upon a fluid flowing along it, whether by adhesion or simple mechanical collision of particles; and this is the usual meaning of the word in this connection. Poisson has considered friction only in its direct action, i. e., as the resultant of the forces *p*, *p'*, &c., of Fig. 3, and, deducing therefrom a deviation to the left, he arrives at the conclusion that the magnitude of the force was not sufficient to account for the *amount* of observed deviation (irrespective, I presume, of *direction*). If this is true, it is difficult to conceive that the force (overlooked by him) arising from developed *pressures* (and which I show to have its origin in friction), is sufficient to annihilate the *direct* effects of friction itself, and to produce the deviations beside. Nevertheless it is all we can account for, and the experiments of Magnus seem to indicate its adequacy.

other,* but the causes they assign are inadequate, and, moreover, as the deviation of an elongated ball depends more on the direction in which its axis happens to be deflected than upon the direction of the disturbing forces, no such uniformity in the deviations as is observed, would be produced, were friction on the inequalities of pressure the governing causes.†

There is another cause which *necessarily* operates in this case; a cause which long before I knew of the experiments of Dr. Magnus, I conceived to be the predominating one.

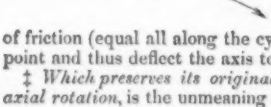
The gyroscope shows, to the apprehension of every one, that a rotating solid of revolution supported by a point in its axis, about which it is free to move, solicited by a force tending to turn it in any direction, turns, *not* in the direction of the force which solicits it, but, with uniform and slow motion, normally to it; that the axis of figure will describe a *cone* about a line passing through the point of support, parallel to the direction of the soliciting force.

The elongated projectile is discharged from the piece with its axis coinciding with its trajectory, but, through the action of gravity, the trajectory deflects from its original direction and from that of the axis.‡ In consequence of this, the resistance of the air acts obliquely to the axis, and, with the ordinary forms of elongated projectiles, without grooves, its resultant passes in front and *above* the centre of inertia, tending to *raise* the point,

* Thiroux's theory requires that in the descending branch of the trajectory, the point of the ball shall be depressed *below* the trajectory; Panot's, that it shall continue parallel to its original direction, and hence elevated *above* the trajectory. Dr. Magnus supposes the axis to keep pretty nearly coincident with the trajectory, and says that experiments made with balls fired at low velocities confirm this assumption.

† To illustrate my meaning, take the friction theory of Panót which supposes the axis elevated above the trajectory and that the friction on the under side (where the air is *densest*) carries it to the *right*; yet were this ball a hollow one (as nearly all are) the tail of the ball would be so much lighter than the point that the forces of friction (equal all along the cylindrical surface) would affect the tail more than the point and thus deflect the axis to the *left*, and produce a deviation in that direction.

5.



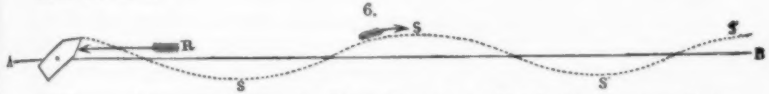
‡ *Which preserves its original direction through the stability it derives from its axial rotation*, is the unmeaning and false expression (or something equivalent) commonly used in this connection. Properly *there is no such thing* as stability of the axis. Take away the possibility of this *conical* motion (in precession) just described, *confine the axis to one plane*, and it becomes as movable as if no rotation existed. This is clearly established in the Analysis of the Gyroscope (this Journal, [2], xxiv, 49), where the following language is used, "the popular idea that a rotating body offers any direct resistance to a change of plane, is unfounded. It requires as little exertion of force (in the direction of motion) to move it from one plane to another, as if no rotation existed." The "surprising phenomena" of "complete mobility" when one of the rings of the gyroscope is so held that the axis is confined to one plane of motion, which Dr. Magnus arrives at *experimentally* (and all the modifications of his experiments) flow directly from my analysis. *If, then*, stability of axial direction is attributed to axial rotation in projectiles, that which is the *inseparable accompaniment* of this *apparent* stability (i. e. the conical motion described in the text), and without which there is nothing even *resembling* stability—must be accepted with it, and the *deviation* of the projectile, which will *certainly result therefrom*.

and from this results the conical motion of the axis to the *right*, if the rotation is to the right—to the *left* in the contrary case.*

Before attempting to apply the foregoing to the real case, it will help us to consider a more simple one.

Suppose the rotating projectile with its axis inclined to its trajectory to be propelled through the air, and that the centre of inertia is *confined* to a rectilinear path (as if it slid along an extended wire, A B, for example), and that the force of gravity does not act.

6.



The resistance of the air, R, acting *immediately* to increase the angle of inclination, produces *instead* the resulting conical motion to the right, which, combined with the motion of translation, would cause the *point* to describe a helix *s s s* about the rectilinear path of the centre of inertia.

Let now all the circumstances be as above *except* the confinement of the centre of inertia.

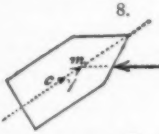
7.

The resistance R, acting obliquely to the axis of figure, will (as in the familiar case of the sailing of a vessel *on* the wind) give the projectile a component of motion in the direction of its axis, and the centre of inertia cannot remain on its rectilineal trajectory A B. But, owing to the conical motion of that axis about a line parallel to the resistance R, (a direction, itself, always changing) the result will be that the projectile *itself* will describe a helix, *s s s*, about the line of original direction A B.†

* These results have been exhibited *experimentally* by Prof. Magnus; they flow directly from my analysis made *without* experiment of any kind.

† The *period* of these conical, or rather helical revolutions, could be computed if we knew the exact intensity of the force R; the distance C *m*, (or γ) from the centre of inertia, at which it cuts the axis of figure; the value *k*, of its radius of gyration about its axis of figure; and the velocity, *n*, of rotation.

8.



The period, T, (as deduced from my treatise on the Gyroscope) would be expressed in seconds thus—

$$T = 2\pi \frac{k^2 n}{\gamma r}$$

In which $r = \frac{R}{M}$ = *total* resistance of the air ÷ mass of the projectile. The value of r for the ordinary musket ball would be about 100. Assuming it at 30 for the elongated ball with more mass and *less* resistance—and assuming γ but $\frac{1}{10}$ inch, putting $n = 2\pi \times 150$, and $k^2 = \cdot 15$ inch, we should get T = $\frac{3}{4}''$ or three-fourths of a second.

If we suppose there to be an actual centre of *resistance*, *m*, (a supposition not true probably, but like that of the meta-centre, in hydraulics, admissible in narrow limits) this period would be *the same*, whatever be the inclination of the projectile. The period would increase as the resistance of the air diminished by loss of initial velocity.

In the foregoing case there is no *permanent* departure, or *deviation* from the original direction of projection; nor would there be much *apparent* departure in the range of the modern rifled arms since the periods of these helical revolutions would probably be but a fraction of a second, while the time of flight is from 6 to 8 seconds.

The matter is much more complicated when, instead of the imaginary case above presented, the force of gravity is introduced and the actual flight of a projectile is considered. The ball is projected, having its axis coincident with the trajectory, but from the very outset of the flight a severance commences in the directions of the axis and trajectory, owing to the action of gravity. The resistance of the air becomes gradually more and more oblique to the axis, pressing (as in Fig. 7) the point upwards, and producing a *precession* (or conical motion) to the right.

The entire character of the motion becomes altered by the constantly shifting direction in which this resistance is acting, owing to the constant *fall* produced by gravity.

It is very difficult, *a priori*, to describe the exact character of the motion. It depends upon unknown facts, and doubtless varies much with the form of the projectile—its velocity of translation and rotation.

It would be going into reasoning probably too difficult to express intelligibly, to attempt to explain why, in all cases, the period of these helical revolutions would be very greatly prolonged, and why I think that in *most* cases (if not all) *no complete* revolution takes place at all, but that the whole motion is confined to the first quadrant. In the latter case the flight of the projectile would exhibit one continuous and constantly increasing deviation to the right. In the case of a considerable number of helical revolutions actually performed, there would still be, with each revolution, an increased deviation to the right, owing to the descending branch of the vibration being so much larger than the ascending* one.

It would, perhaps, be unprofitable—in the absence of any sufficient data—in the absence even of experiments made with any knowledge of the *real* causes of deviation, and therefore nearly useless as reference for data, to pursue this subject any further.

That the *precessory* motion is an inseparable attendant to that (so called) *stability* of axis which is the *very object* of giving rotation, I have shown; that it is an *adequate* cause of deviation I think will be admitted. Hence, while *other* causes *may* contribute to the effect, or may oppose it (according to the shape of the ball), this peculiar effect must be looked upon as the controlling cause of deviation of elongated balls discharged from rifled arms.

* In this case the helical path would probably be so *prononcé* as to be easily detected by firing through screens.

I will but add a few very general conclusions I have arrived at from the foregoing.

First, the elongated ball possesses, from *its very shape*, a tendency to pursue the *direction* of *its axis*. This tendency should be made available, so far as practicable, to sustaining the flight, and flattening the trajectory.

Second. In the modern improvements in the art of throwing projectiles from rifled fire-arms, a decided step has been made *backwards*, in losing that most essential element to range and accuracy, *initial velocity*. It is desirable, and, I should think, not impracticable to restore it.

The initial velocity of the old rifle ball (weighing but about 180 grains) was 1750 feet per second. To the modern projectile, weighing about 500 grains, is given but from 900 to 1000 feet initial velocity.

It would doubtless be difficult to *give* the high velocity to so heavy a ball, and if given, the recoil would be inadmissible. But why throw so heavy a ball?—and why adhere to such calibres? The long range of such balls is not due to their weight, but to their *model* and to the *low ratio of their cross section to that weight*.

Both these advantages can be attained with a light ball as well as with a heavy one.

An increase of weight over that of the old musket ball is, *in itself*, objectionable, by increasing the soldier's load.

I can see no reason why, to a ball of the weight of the old musket ball (340 grains), may not be given all the properties of the heavier ones, with the additional great advantage of a *high* initial velocity, approximating to that of the old rifle balls.

To accomplish this, of course, the calibre must be greatly reduced—the ball elongated, and a *comparatively* large charge used (less, however, than that used in the old musket—viz. 110 grains).

This highly elongated ball, *balanced* (if I may use the term) by grooves around the after end, so arranged that the resultant of the air's resistance shall pass as nearly as practicable through the centre of inertia,* will fulfill likewise the *first* condition I

* According to my theory of the matter, these grooves, instead of acting like an arrow's feathers, to keep the axis coincident with the tangent to the trajectory, *have just the contrary object*, to keep it (if properly adjusted) parallel to that original direction, by so balancing the forces of resistance as to cause their resultant to pass through the centre of inertia. The theory of "arrows' feathers" supposes, on the contrary, that, by the grooves, this resultant is thrown *behind* that centre, and tends to *tilt* the axis; an operation which must inevitably, *likewise*, produce *deviation*. Prof. Magnus' observation on balls with low initial velocities (so as to be visible to the eye) confirm my position, though I attach little value to those observations.

Experience seems to show that this balancing of the forces of resistance may be produced *without* grooves (vide Wilcox, p. 160), for the Enfield ball and the Whitworth projectile seem to be without them, as likewise Lancaster's. I have seen no statements, however, as to the *deviation* of these projectiles.

have laid down. For the highly elongated ball possesses in a proportional degree the tendency to pursue the direction in which its axis points: and the causes of deflection of the axis' direction being eliminated by a proper balancing about the centre of inertia of the forces of resistance, the tendency is to pursue the *original* line of direction, in opposition to the downward curvature due to gravity, and thus to *flatten* the trajectory and increase the range.

These conclusions are those of theory alone, but, if I mistake not, all the most recent advances in rifled arms have been in the direction which they indicate. The Swiss (Federal) rifle, one of the most perfect in Europe (see Lieut. Wilcox's "Rifles and Rifle Practice," p. 187) has a calibre of but 0·41 (inch), and its ball, 2½ calibres in length, weighs but 257 grains, thus combining with the small calibre, the highly elongated form, and even then weighing *less* than our old spherical musket ball. Though its initial velocity is not given, yet as the greatest *proportional* charge of powder is used with it, doubtless it also receives the *highest initial velocity*,* of any of its class of projectiles now known in any service.

The *hexagonal projectile* of Whitworth is another instance in confirmation of the principles I advance.

Of small calibre and highly elongated,† he throws this projectile with such accuracy as to hit with certainty, at 500 yards, a disk *not more than two inches in diameter;* and "asserts that he will not rest satisfied till he has fired a ball from one of his guns into the barrel of another, at a distance of 500 yards." (Edinburgh Review, Ap. 1859.)

The "Armstrong" projectile is another characteristic instance. So decidedly is the *elongation of the ball* characteristic of the most recent and successful efforts in obtaining range and accuracy, that the English writer just quoted applies the term "bolts" to the Whitworth and Armstrong projectiles.

In fact, the *two* springs from which have risen the modern improvements in projectile weapons are 1st, the application of the *rifled principle* to *all arms;* 2d, the *elongation* of the projectile. *Either one alone* may produce, *to a certain extent*, the results desired; it is only by the best possible combination of the two that the best results can be educed. The *increase of range* is due

* Initial velocity is the *very first* element in procuring range and accuracy. The greater it is, other things being equal, the *flatter* will be the trajectory (one of the principal elements of accuracy), and the flatter the trajectory the less the lateral deviation laterally. The extreme curvature of trajectory (though it may have its advantages in some peculiar circumstances), is the great difficulty as to accuracy *of fire*, in modern rifled weapons.

A *necessary evil* with the heavy balls used; the pretence that it is, *in itself*, an advantage, is too absurd for controversy.

† According to Wilcox the mean calibre is about 0·46, and its length 2½ calibres.

almost entirely to the latter principle, and it is only by applying it to the utmost *practicable* extent (as in the case of the Whitworth and Armstrong projectiles), that the greatest range and most perfect accuracy can be obtained.

The *calibres* in use have been a positive *bar* to the successful use of this principle in small arms. Borrowed from the old smooth-bored weapons, the adherence to them has caused an unnecessary *increase* of weight,* and made a loss of *initial velocity* inevitable, with all the attendant evils of a highly curved trajectory, and large deviation.

"Notwithstanding" (says Wilcox) "the long time that has elapsed since the discovery of the rifle, its principle is not yet so well understood as to have led to the general adoption of any particular form of this arm as the best."

The above conclusion of the author of "Rifles and Rifle Practice," will justify me, I hope, in venturing to make the foregoing suggestions on the subject.

New York, Dec. 15, 1859.

ART. XVIII.—*Gulf Stream Explorations—Third Memoir. Distribution of Temperature in the Water of the Florida Channel and Straits;* by A. D. BACHE, Sup't. U. S. Coast Survey.—With Diagrams.

(Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.)

The results of the explorations of the Gulf Stream in the survey of the Coast, have been communicated to the Association from time to time, as phenomena of peculiar interest have been developed.

The original plan of these explorations having been carefully studied, and having proved successful, has steadily been adhered to. The more recent observations have been directed to that part of the stream between Havana and Cape Florida, known as the channel and strait of Florida.

I have now to present four sections showing the depth and temperature in this most important region of the Gulf Stream. These results are from the observations of Commander B. F. Sands and Lieut. Commanding T. A. Craven, U. S. Navy, Assistants in the Coast Survey, whose names have already been mentioned before the Association in connection with explora-

* The old spherical musket ball of ·69 calibre weighed 340 grains. The new "rifle musket" ball of ·58 calibre weighs 500 grains, while still heavier balls are in use in Europe. Thus we see in use balls surpassing in weight the musket ball, and having *twice*, and even *three times* the weight of the Swiss ball described in the text, admitted to be, practically, the best in Europe.

tions of the Gulf Stream, and furnish a sufficient guaranty that the results have all the reliability which care, experience and zealous labor could give them.

Section No. 1, from Cape Florida to Bemini was run by Lieut. Commanding Craven, in May, 1855; Section No. 4, by Commander Sands, in May, 1858; and Sections 2 and 3 by Lieut. Commanding Craven, in April and May of the present year, (1859). Sections 2, 3 and 4 are perpendicular to the direction of the Stream at distances of about fifty, one hundred, and two hundred miles from Cape Florida. The Florida strait is funnel-shaped, being about ninety miles wide at Havana and about forty-five miles wide at Cape Florida, the narrowest part.

Form of bottom.—The area of the water way and the form of the bottom are represented on diagrams 7, 8, 9 and 10. The Arabic numerals at the top represent distances from the Florida coast (the Keys) in miles, and the Roman numerals, the positions at which observations are made. The numbers at the left hand represent the depth in fathoms.

Commencing at the Cape Florida section, it will be seen that there is a rapid descent of the bottom to the Havana section, from three hundred and fifty fathoms to eight hundred fathoms, or twenty-seven hundred feet in a distance of two hundred miles. The most shallow as well as the narrowest part of the Stream is therefore at Cape Florida. The deepest water follows the coast of Cuba and the Grand Banks, the depth being eight hundred fathoms at a distance of only five miles from Havana, nearly four hundred fathoms within five miles of Salt Key Bank, and three hundred fathoms close to the island of Bemini. The descent from the Florida side is for the most part gradual, but from the opposite side abrupt. This effect seems to have been produced by the action of the sub-current in wearing a deeper channel upon the concave side of the Stream. At Havana there is an abrupt descent of nearly a mile within five miles of the shore, while on the side of the Tortugas and Key West the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf Stream makes the circuit of the Gulf of Mexico, since, if it impinged directly upon the island of Key west and the Tortugas we should find its effects in the wearing of a deeper channel on that side.

TEMPERATURES.

Change of temperature with depth.—In a former communication the law of change of temperature with depth was discussed, and types of the curves representing the law were given for different parts of the Stream. The curves were all merely modifications of a more general form. Thus, the cold water between the Gulf Stream and the Coast gave one form; the axis of the Stream

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DIAGRAMS SHOWIN

of the

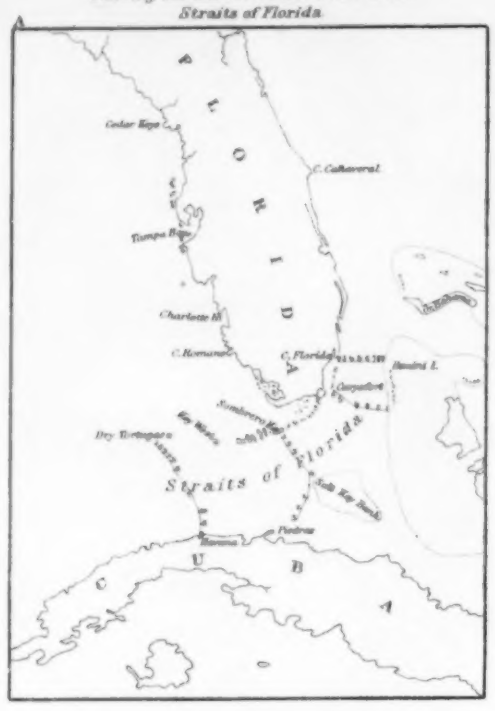
GULF STREAM EXP

From

Observations made in 1845 '46

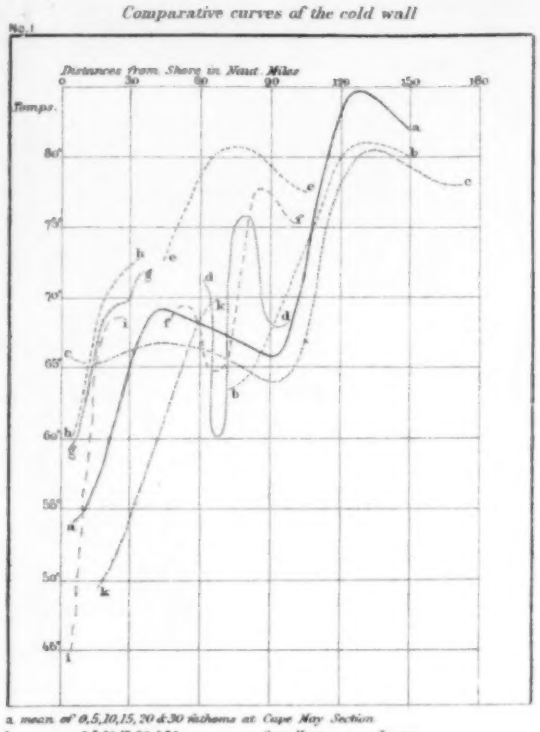
1859

CHART
showing the lines of Sections run in the Straits of Florida



Comparative curves of the cold wall

No.1

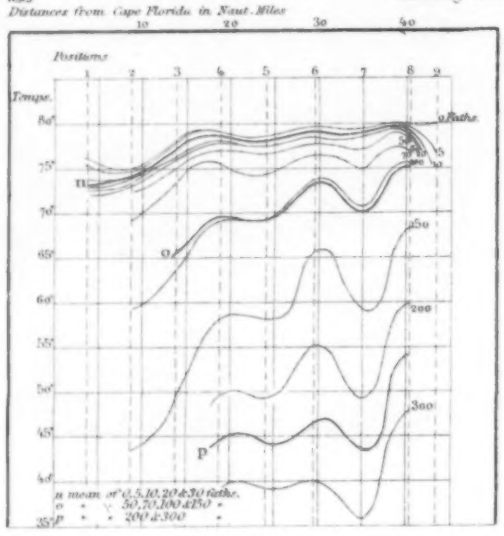


a. mean of 0,5,10,15,20 & 30 fathoms at Cape May Section.
b " " 0,5,10,15,20 & 30 " " Cape Henry " 3years
c " " 0,5,10,15,20 & 30 " " Sandy Hook "
d " " 20 & 30 " " St. Simon's "
e " " 0,5,10,15,20 & 30 " " Cape Fear "
f " " 20 & 30 " " Charleston "
g " " 100 " " Cape Florida "
h " " 100 " " Carysfort Lt.Ho.
i " " 70 & 100 " " Sombrero Key "
k " " 200 " " Tortugas "

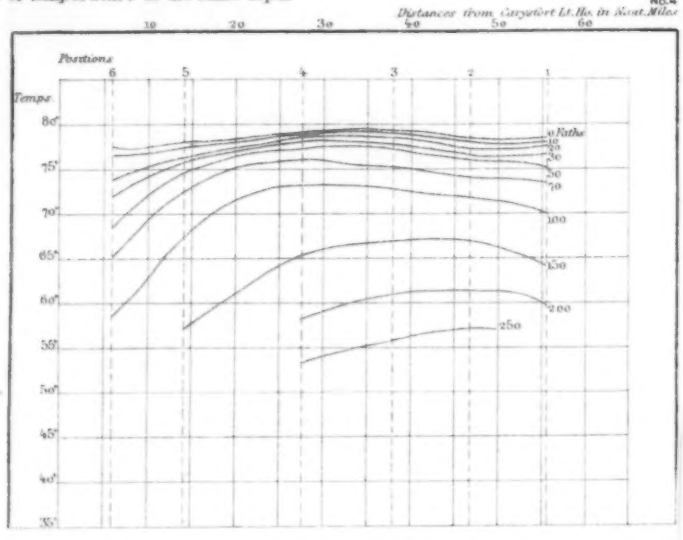
Sections off Cape Florida and Carysfort Lt.Ho. Diagrams of Sections

Showing changes of temperature at the same depth

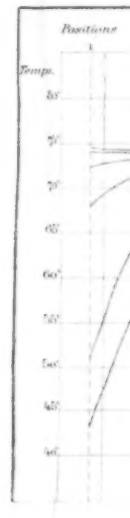
No.3
Distances from Cape Florida in Naut. Miles



No.4
Distances from Carysfort Lt.Ho. in Naut. Miles



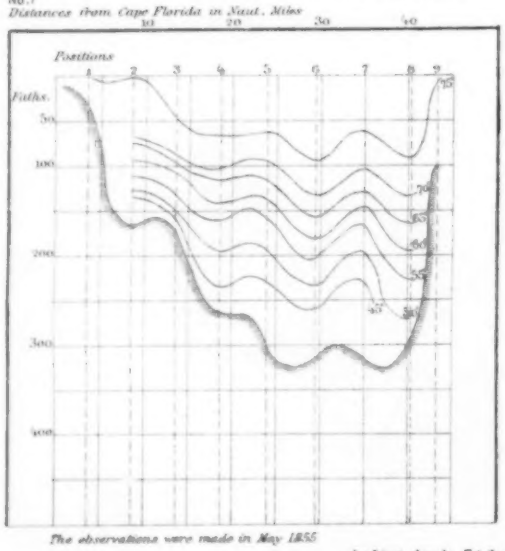
No.5
Distances from S



Section 1 **Section 2**

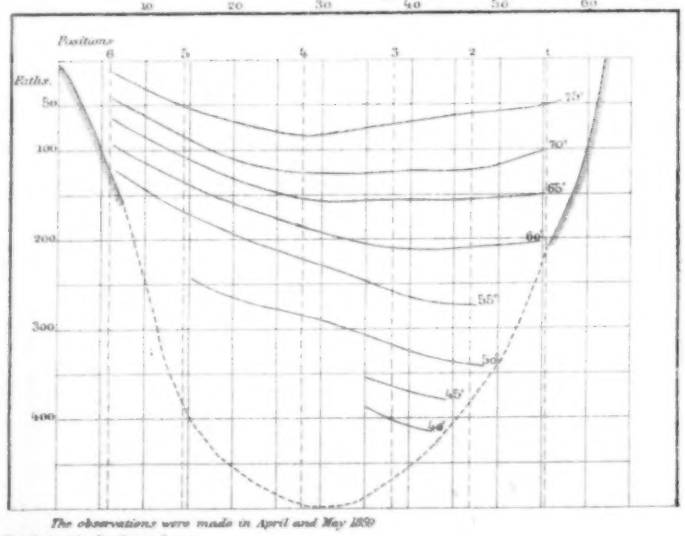
Showing bottom and changes of depth for the same temperature

No.7
Distances from Cape Florida in Naut. Miles



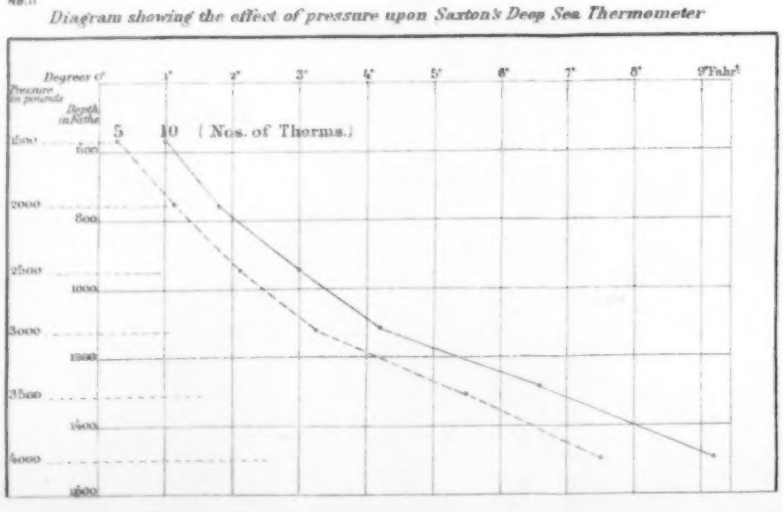
The observations were made in May 1855

No.8
Distances from Carysfort Lt.Ho. in Naut. Miles



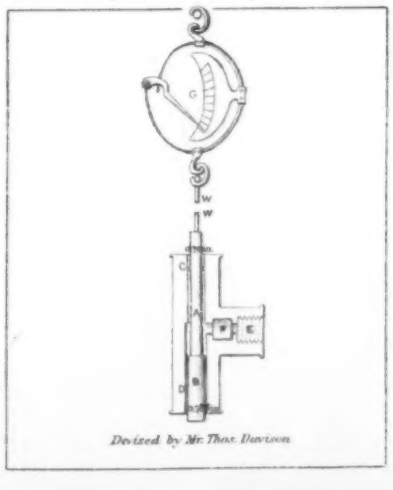
The observations were made in April and May 1859
by Lieut. Comdg. T.A.Craven U.S.N. Asst. in the Coast Survey

No.11
Diagram showing the effect of pressure upon Saxton's Deep Sea Thermometer



GAUGE
for measuring the pressure in hydraulic cylinders

No.12



Devised by Mr. Thos. Davison

OAST SURVEY

BACHE Supdt.

OWING THE RESULTS

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AM EXPLORATIONS

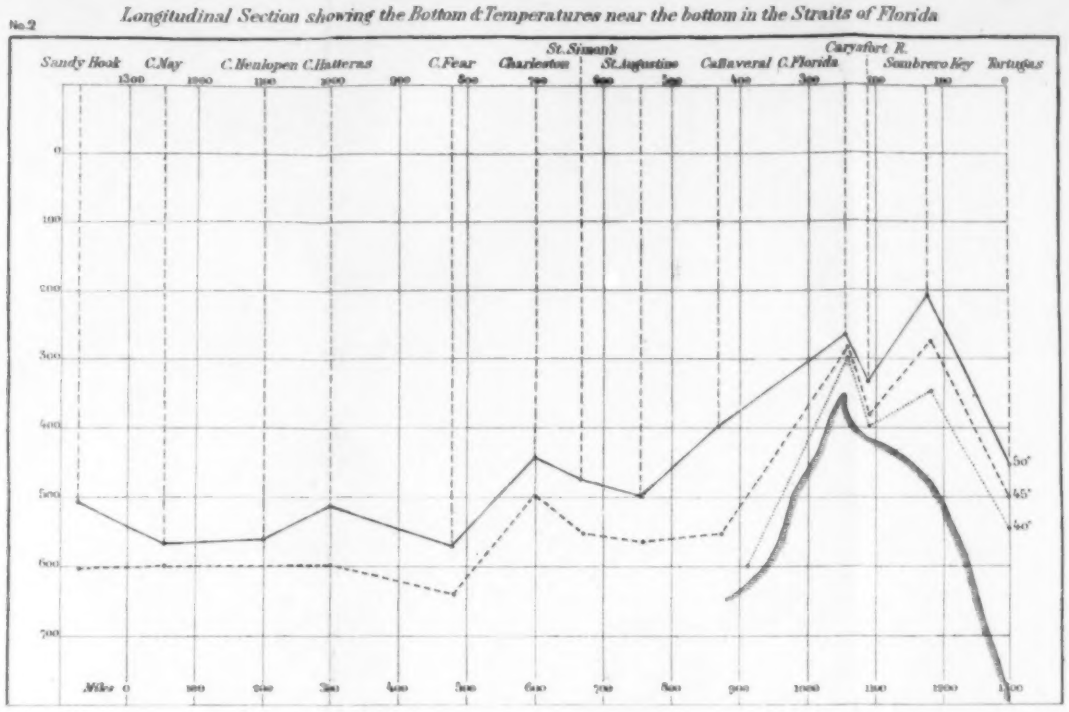
From

e in 1845 46 47 48 53 55 58 & 59

1859

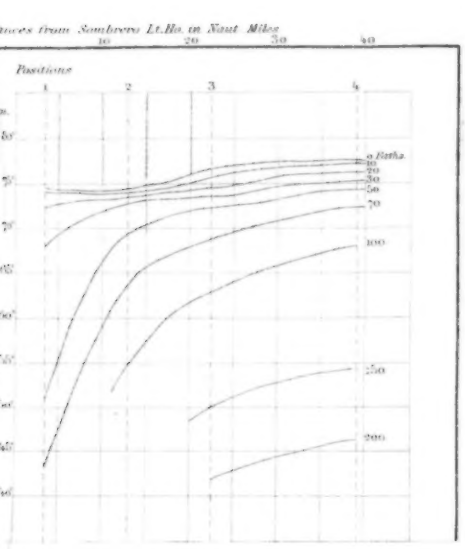
No.2

Longitudinal Section showing the Bottom & Temperatures near the bottom in the Straits of Florida



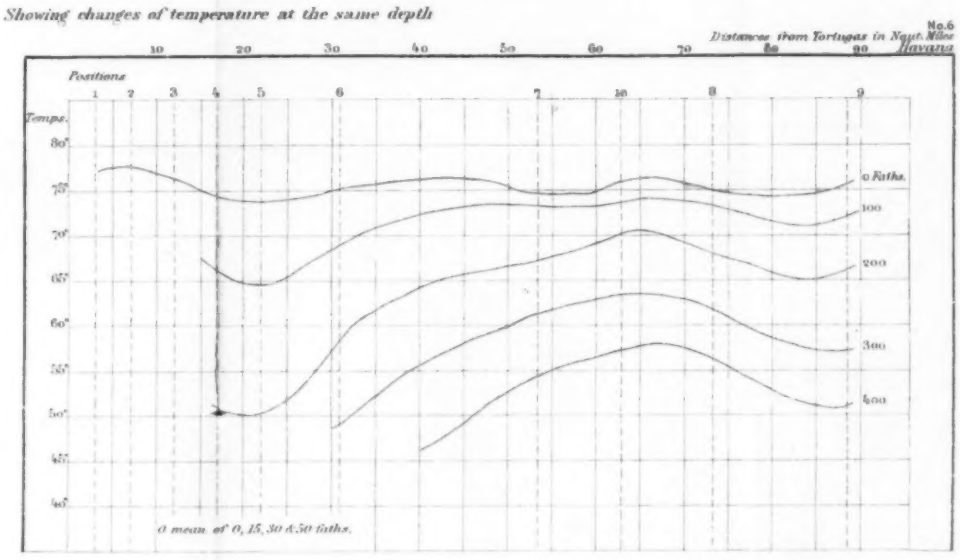
Sections off Sombrero Lt. Ho. and Tortugas. Diagrams of Sections

Showing changes of temperature at the same depth



No.6

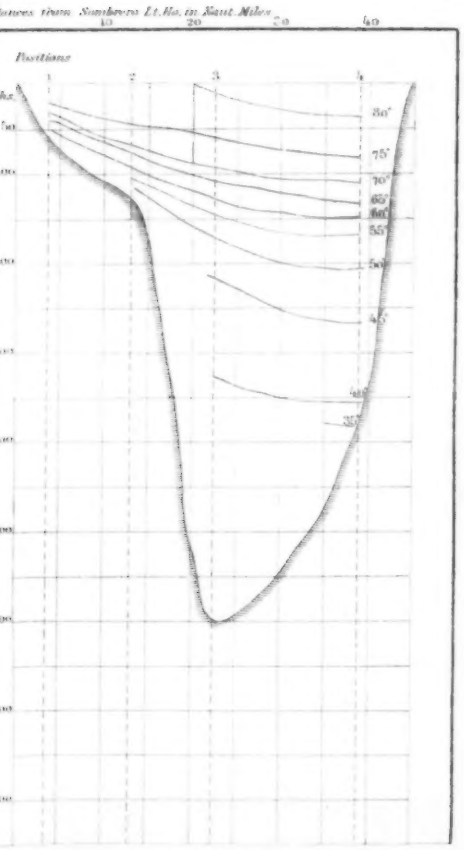
Distances from Tortugas in Naut. Miles



Section 3

Section 4

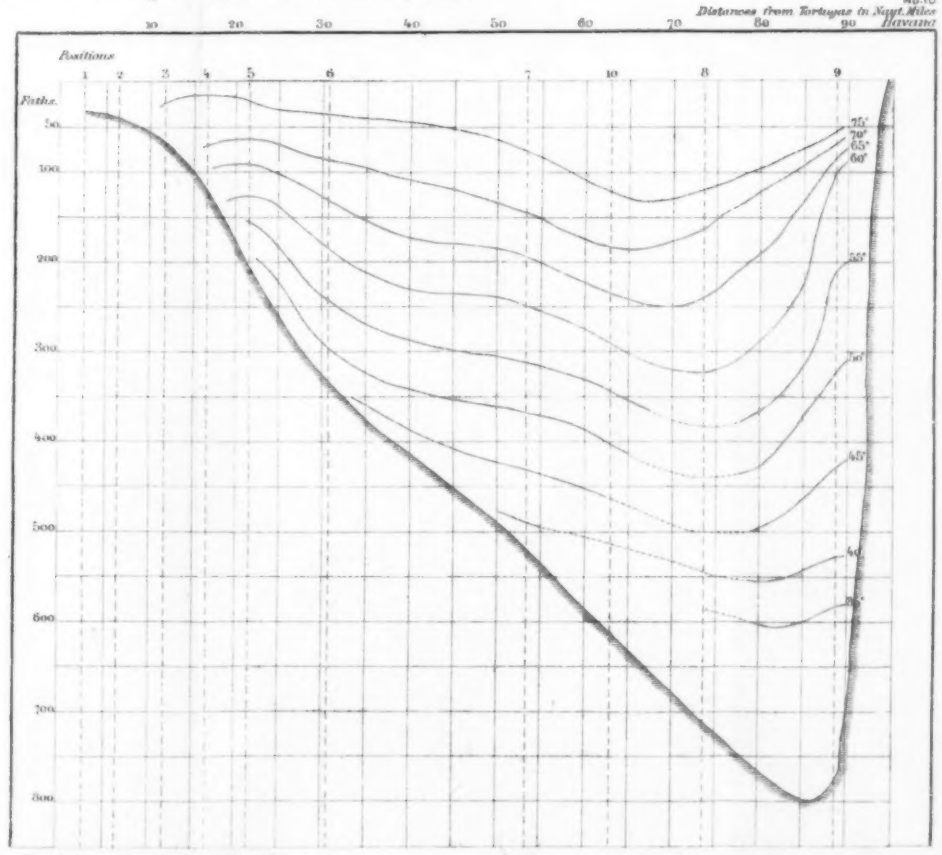
Showing bottom and changes of depth for the same temperature



The observations were made in April 1859

No.10

Distances from Tortugas in Naut. Miles



The observations were made in May 1858

by Commander B.F. Sands U.S.N. Asst. in the Coast Survey

another; and the water beyond the axis a third form, while in the strait of Florida a fourth was developed. It would be natural to expect in the course of many years' explorations by different individuals with different instruments not even of the same class, that general phenomena of this character should present some contradictions and some inexplicable results. Experience however has confirmed the first conclusions and the constancy of the phenomena. It is not difficult, having the curve representing the temperatures at any position from the surface to the depth of several hundred fathoms to determine from the temperatures alone, in what part of the Stream they were taken.

Temperature in a direction perpendicular to the Stream.—Diagrams 2, 3, 4 and 5, show the changes of temperature for the same depth in each of the sections, and diagrams 7, 8, 9, and 10, the depth for the same temperature.

Bands of warm and cool water.—In the section from Cape Florida to Bemini, the division of the Stream into bands is plainly exhibited, though more faintly than in the northern sections, and the form of the bottom in this section shows the elevations and depressions corresponding to the divisions. In the sections south of Cape Florida, all traces of the bands seem to disappear as well as the ridges of the bottom. The bands therefore seem to have their origin near Cape Florida, and the conclusion stated some years ago, as the probable one, is strengthened, that they are caused by the ridges and valleys of the bottom parallel to the general course of the Stream, and along which the Stream and counter stream have their course.

The Cold Wall.—The cold wall, as an exception to the remark made above in reference to the bands, is traced as far as the Tortugas, and is plainly shown in all the sections with more or less distinctness. In the Sombrero Key section (No. 3) it is strongly marked at depths ranging from seventy to a hundred fathoms, while in all the sections the warm water at the surface overflows the cold wall and reaches quite to the shore.

Diagram No. 6 represents the comparative curves of the cold wall in different sections of the Gulf Stream, including those in the Straits of Florida. The figures at the top show the distances of the cold wall from the shore in the different sections, and the numbers on the left the degrees of temperature. The curves are drawn for different depths in the several sections, as shown in the notes at the bottom of the diagram. The curves g, h, i, k, represent the cold wall in the four sections under consideration.

Longitudinal Sections.—It has been found very difficult to deduce any satisfactory law for the decrease of surface temperature along the axis of the Stream owing to the variability of the temperature of the water of the regions from whence the Gulf Stream is supplied. Two modes of investigating the subject

have been pursued, one by following the stream from the Gulf of Mexico, and making hourly observations of the temperature of the water, and the other by comparing the mean temperatures of the various sections with each other, and with the temperature of the Gulf of Mexico. In the first method, the vessel must be allowed to drift with the current of the stream, a difficult condition except in the best weather, even for a day, and to float along thus, for hundreds of miles would rarely be practicable. Any motion communicated by sails or by steam must carry the vessel beyond the water in which she commenced her voyage, and the lateral overflow carries the water constantly from the axis toward the edges of the Stream.

In the comparison of mean temperatures of the different sections, the fact has been established, that the temperature of the water of the Stream at any point may be higher than at a point nearer the source, and hence vessels in running along the Stream may, and generally do, pass through water not of a constantly diminishing temperature, but from cool to warm, and the reverse. This is to be explained mainly though not entirely, by the variability of temperature at the source.

By taking the mean temperature of any one section, and going back to the date of the departure of the waters from the Gulf of Mexico, as determined by the velocity of the stream, and comparing the temperatures observed with the temperature of the Gulf waters, it was supposed that a solution of the question might be obtained. The temperatures were taken from the most authentic meteorological records of the Gulf for a series of years, and those periods sought which corresponded to the dates desired. The uncertainty of the temperatures of the waters of the Gulf of Mexico, as obtained from air temperatures taken here and there along its shores rendered the results unsatisfactory. Enough seems to have been determined, however, to show that the surface temperature of the Gulf Stream along its course is variable; that a vessel sailing along the axis at a more rapid rate than the motion of the stream, will pass through water of higher and lower temperature, depending generally upon two conditions, viz: the distance from the Gulf of Mexico, and the temperature of the Gulf at the time the water entered the straits of Florida; and further, that the latter cause is the predominating one in the parts of the Gulf Stream adjacent to the Atlantic coast where the current is rapid.

The influence of the form of the bottom in forcing the cold counter current of the bottom upward, has been adverted to, and the fact appears to be well established in the cross sections where the ridges and valleys parallel to the direction of the stream separate it into bands of warmer and cooler water, and this conclusion, as has just been stated, is strengthened by the fact that the

bands and ridges simultaneously disappear south of Cape Florida. The phenomenon is moreover strikingly exhibited in the longitudinal section of the bottom, in connection with the lower temperatures.

The shallowness of the Stream in the strait of Florida, connected with the fact that the bottom falls off rapidly to the north and south afforded an excellent opportunity for testing the question. If the cold water of the under polar current follows the bottom, it should appear in the shallow part of the strait, and here the warm water of the surface, and the cold water of the bottom, would approach each other. Diagram No. 1 shows the curves of 40°, 45°, and 50° (bottom temperatures) along the deepest part of the stream, commencing at Sandy Hook, and running as far as the Tortugas. All these curves rise with the bottom and pass over the ridge which divides the bed of the Atlantic from that of the Gulf of Mexico, and again fall with the slope of the bottom towards the Gulf. In the narrowest part of the strait where the depth is three hundred and fifty fathoms, the temperature from the surface to the bottom, ranges between 80° and 40°.

On the effects of pressure on Saxton's deep sea thermometer.—In the exploration of the Gulf Stream, the temperatures below one hundred fathoms have mostly been determined by Saxton's metallic thermometer, and although the results have been consistent amongst themselves, and have agreed well with the indications of other thermometers, yet it was thought advisable to determine the effect of pressure by direct experiment.

Saxton's thermometer consists essentially of a compound ribbon of silver and platinum fused and pressed together by rollers. This ribbon is wound in a spiral form, one end of the spiral being firmly fastened to an interior solid axis and the other left free. Upon the free end is placed an index arm which moves over a circular graduated scale carrying with it a friction hand or indicator which is left at the extreme point of the arc reached by the true index. The instrument is enclosed in a case to which the water is freely admitted. A variation of temperature is immediately noticed, as the effect is to give a rotary motion to the index.

The experiments to determine the effect of pressure were made at my request by Mr. J. M. Batchelder with means devised by Mr. Thomas Davison at the Novelty Iron Works. The following description of the apparatus employed, is given by the last named gentleman.

"The gauge consists of a brass cylinder H, about eight inches long into which a steel plunger is fitted, the upper part of the plunger at A being ·70 of an inch in diameter, and the lower at B about ·786, so that the difference in area of the ends is equal

to one tenth of a square inch. The cylinder is bored out a little larger than the plunger except for about a fourth of an inch near each end at C and D where both are accurately fitted. To the branch E a pipe connects, communicating with the hydraulic cylinder and leading the water into the centre of the gauge which it reaches after passing through the chamber F filled with sponge to prevent any impurities in the water from reaching the plunger. The upper end of the plunger connects by a wire W, to a spring as shown in the sketch at G, so constructed as to indicate pressure from 0 to 450 lbs., the spring being so strong that 450 lbs. produce a movement of the plunger equal to three-eighths of an inch. It is evident that as the difference in area of the ends of the plunger is one-tenth of an inch, one hundred pounds pressure from the water on this surface, as indicated by the balance, would equal a pressure of water of 1000 lbs. per inch, or a pressure ten times as great as that indicated by the balance throughout its scale. The only difficulty in the use of the gauge is that of fitting the plunger to the cylinder so that while it is perfectly free to move it is also perfectly water tight. This difficulty however has been overcome, and much advantage was also derived from Mr. Batchelder's suggestion for supplying the wear of the plunger and cylinder by depositing brass on the plunger through the galvanic process."

Connected with this gauge by a pipe is a strong wrought iron cylinder, sixteen inches long by four inches in diameter, in which the thermometer was placed, the opening being firmly closed by a screw plug. This second cylinder was immersed in a tub of water for the purpose of regulating the temperature. The thermometer once placed in the cylinder, is not again removed, the index being read by means of a mirror until the observations are completed. By the use of this apparatus, the effect of pressure up to 4000 lbs. per square inch was observed upon two thermometers, and the results are given below. The observations were made to indicate the effects of 500, 1000, 1500, 2000, 2500 lbs. pressure, etc. Seven series of experiments were made with thermometer No. 5, and five series with No. 10. The mean results show that a pressure of 1000 lbs. per square inch has no effect upon the thermometer; at 1500 lbs. the effect is less than one degree; and from 1500 to 4000 lbs. per square inch, the effect is to diminish the readings, the maximum effect being seven degrees.

The diagram exhibits the law of diminution by increase of pressure, and the depth corresponding to different pressures. The correction to be applied varies with the depth. For thermometer No. 5 it is only four tenths of a degree Fahrenheit at the depth of 600 fathoms. For thermometer No. 10, it is one degree at the same depth.

At 1500 fathoms the corrections are respectively five and a half and seven degrees.

Nearly all the temperatures observed in the Gulf Stream have been taken at depths less than six hundred fathoms.

Table showing differences of readings of Saxton's Thermometer under pressure and free from pressure.

Thermometer No. 5.							Thermometer No. 10.						
No. of Series.	Pressure in Pounds.						No. of Series.	Pressure in Pounds.					
	1500.	2000.	2500.	3000.	3500.	4000.		1500.	2000.	2500.	3000.	3500.	4000.
	°	°	°	°	°	°		°	°	°	°	°	°
1	1·6	0·	3·75	0·	0·	0·	1	0·	2·0	3·25	4·5	6·25	8·25
2	0·	1·00	2·	2·8	4·5	5·5	2	2·0	1·0	3·5	4·5	6·0	7·25
3	0·	1·0	2·25	3·75	4·75	5·7	3	0·75	2·0	3·0	3·25	5·5	6·5
4	0·	0·5	0·50	2·0	3·6	5·5	4	1·75	2·0	3·5	4·75	5·5	7·25
5	0·	1·75	2·25	3·5	5·0	6·5	5	·75	1·75	1·75	3·75	5·0	6·75
6	0·	1·25	2·25	3·75	6·0	6·5	Means	1·00	1·75	3·0	4·25	5·6	7·25
7	0·5				3·5	4·25							
Means	0·3	1·1	2·1	3·2	4·5	5·6							

Art. XIX.—*On the Chemical Composition of Pectolite;* by J. D. Whitney.

A few years since I made some examination of specimens of a radiated fibrous mineral from Isle Royale, Lake Superior, which proved on analysis to be pectolite. A mineral, closely resembling pectolite, from Bergen Hill, New Jersey, which had been analyzed by L. C. Beck, and considered by him as identical with the stellite of Thomson, was examined at the same time and found to agree in composition with pectolite, as had been previously suggested by J. D. Dana. Both the stellite and Wollastonite of Thomson were referred by me, at that time, to pectolite,* a reference the correctness of which has since been shown by Messrs. Heddle and Greg, in a paper on the composition of the English varieties of this mineral.†

Notwithstanding so many analyses of pectolite have been made by different chemists, there has not been a sufficient accordance in the results obtained to justify a positive decision as to the real formula of the mineral, although that of Von Kobell has been generally adopted. It will be sufficient to refer to the various published analyses, to see that there is but an unsatisfactory degree of uniformity in their results, whether of specimens from American or European localities. Thus, for instance, in

* Journal of Boston Nat. History Soc., vi, 40.

† Philos. Mag., [4], ix, 238; also in Erdmann and Marchand's Journal, lxvi, 144.

Von Kobell's analysis of the Monte Baldo pectolite, the silica is given at 51·3 per cent, while other analyses of Scotch and American varieties give as much, in some instances as 54 and 55 per cent of that substance. In the like manner, the amount of lime, as stated by different analysts, varies from 29·8 to 35·2 per cent, while there is even less agreement in the water, which is given at from 0·41 to 3·39 per cent.

The difficulty of procuring, in a perfectly pure state, a mineral which only occurs in a finely-fibrous condition is undoubtedly one of the principal causes of these discrepancies in the analyses; but it is also possible that the unusual care required for the correct determination of the silica in the very soluble class of minerals to which pectolite belongs may not, in all cases, have been appreciated. The great abundance and purity of the specimens of this mineral which have been obtained from the tunnel of the Erie railroad, recently excavated through Bergen Hill, seemed likely to obviate the first difficulty mentioned above. The results of three analyses indicated that this material was really of almost absolute purity, while no pains were spared to effect a complete and accurate separation of the various ingredients, and especially of the silica.

The pectolite dissolves more or less completely in chlorohydric acid, according to the strength and quantity of the latter. By using a considerable excess of rather dilute acid, all, or nearly all, the pulverized material may be dissolved into a clear liquid. As the attack is usually performed, a portion of the silica remains in solution and the remainder separates as a flocky precipitate.

The following experiments show the difficulty of estimating the silica correctly in this class of highly soluble silicates, and the necessity of unusual precautions in its determination.

On digesting the *ignited* mineral with acid until a perfect attack seemed to have taken place, the solution gelatinized on evaporation, and there was no perceptible gritty feeling when it was stirred with a glass rod or the spatula; on separating the silica, however, after evaporating to dryness, moistening with acid, and adding water, in the usual way, its amount was found to be equal to 62·10 per cent of the substance taken.

Another portion of the *unignited* mineral was attacked by acid, and the silica separated without evaporating to entire dryness; its amount equalled only 35·6 per cent. To procure the whole amount of this substance present in the mineral, and uncontaminated by any traces of the bases, it was found necessary to use the unignited substance for the attack with acid, carefully to evaporate to entire dryness over the water-bath, then to moisten the dried mass with strong acid and allow it to stand for some time before adding water and filtering. These precau-

tions will give a perfectly pure silica, but not all of it, as two or three per cent will still remain in the solution, a part of which will go down with the ammonia precipitate, and the remainder be found after driving off the ammoniacal salts, (the lime having been previously separated,) and igniting the residuum.

The following are the results of three analyses of as many different specimens of the pectolite from the Bergen Hill tunnel.

	I.	II.	III.
Silica,	54·82	54·76	54·27
Lime,	33·12	32·88	32·83
Protoxyd of manganese,	·66	1·16	1·24
Protoxyd of iron,	·26		
Soda,	8·78	9·17	8·94
Water, by loss,	2·36	2·03	2·72
	100·00	100·00	100·00

The direct determination of the water on the substance dried at 80° C. gave, for II, 3·03, and for III, 2·75 per cent. Specimen III, from the Wheatley Collection, Union College, was apparently the purest; it was a fragment of a mass, the fibres of which were several inches in length, slightly divergent from a common centre, and being nearly transparent and evidently quite free from any admixture with quartz or any other foreign substance.

In analysis III the oxygen is as follows:

Silica,	28·942
Lime,	9·379
Soda,	2·306
Protoxyd of manganese,	·290
Water,	2·44

This gives as the ratio:*

	$\dot{H}$	:	$\dot{N}a$	:	$\dot{C}a$	:	$\ddot{S}i$
	1	:	1·05	:	3·83	:	11·84
or nearly	1	:	1	:	4	:	12

If we attempt to express this ratio by a formula, we have

$$\dot{N}a^3\ddot{S}i^4 + 4\dot{C}a^3\ddot{S}i^3 + 3\dot{H}.$$

The percentage demanded by this formula is given below, by the side of that required by Von Kobell's.

	Whitney.	Von Kobell.
Silica,	54·22	52·05
Lime,	33·73	35·33
Soda,	9·33	9·77
Water,	2·74	2·83

It is evident, from a comparison of the figures given above, that the formula now suggested agrees more nearly with the re-

* The atomic weights used are as follows:

Na=23 : Ca=20 : Si=21, H being =1.

sults of the analyses of the Bergen Hill pectolite than any yet proposed. As written here, its relations to that of spodumene are to be noticed, as also to those of Wollastonite and pyroxene. The latter connection will be made plainer by writing the formula of pectolite thus:

$$\dot{R}^3 \ddot{S}i^2,$$

$\dot{R}$ being $(\dot{C}a\frac{4}{5} + \dot{N}a\frac{1}{5} + \dot{H}\frac{1}{5})$.

Northampton, Mass., Feb. 1859.

ART. XX.—*Notes on the Ancient Vegetation of North America;* by Dr. J. S. NEWBERRY. In a letter to Prof. DANA, dated Santa Fe, New Mexico, Oct. 15th, 1859.

Dear Sir:—I have just returned to Santa Fe after an absence of three months, spent in an examination of the geological structure of the country bordering the San Juan and Upper Colorado rivers, in Utah and New Mexico, connected with the War Department topographical survey under Capt. Macomb, Topog. Engrs.

The region visited proved interesting in many respects—beautifully picturesque and unexpectedly productive—covered with ruins, and once densely populated by a race that has now entirely abandoned it.

I would cheerfully give you a sketch of its remarkable physical and geological structure, but the results of the expedition will doubtless be published in detail by the War Department, and it is not proper that any part of them should now be given to the public. I will say however, in general, that our field of exploration includes an immense labyrinth of great cañons, scarcely less abysmal than those of the Lower Colorado in which we were last year involved—some of which are over a mile in depth—and even more varied and wonderful in character. The sections exposed in their walls permitted me to measure and examine all the strata between the base of the Carboniferous and the summit of the Cretaceous series; the latter formation attaining a thickness of 4000 feet, and occupying an immense area west of the main divide of the Rocky Mountains.

Our work this season connected on the south with that of the party with which I was associated last year under Lieut. Ives, T. E.—and combining the results of both expeditions, I have now a complete and detailed section of all the rocks composing the great central plateau of the continent, from the base of the palæozoic series to the summit of the Cretaceous. These strata are conformable throughout, and over 10,000 feet in thickness.

The collection of fossils made, both animal and vegetable, is quite large, and, with considerable new matter, includes what is much better, many well known species of which the geographical range will be seen to be much greater than has been heretofore supposed.

I was agreeably surprised to find here on my arrival, the May, July and Sept. numbers of the Journal, published during my absence, by which I have been in a degree placed *au courant des affaires scientifiques*. In the articles in the July number from the pen of my friend Mr. Lesquereux, I have been much interested, as they refer to matters which have engaged much of my attention for some years. In the letter of Prof. Heer there is however a passage which seems to me to require notice; although, situated as I am, without specimens or books for reference, I am scarcely prepared to take up the discussion of the questions involved in it. This is the less necessary now, as what I have to say in reference to them will be found *in extenso* in the reports on the geology of the country west of the Mississippi which I have made or have in preparation for the general government.

The paragraph to which I allude is as follows:—

"Your views of the gradation of the flora of North America agree perfectly with what we find in Europe. This led me to believe that the plants of Nebraska belong to the tertiary and not to the cretaceous formation. It is true that I have seen only some drawings which were sent to me by Messrs. Hayden and Meek; but they are all tertiary types. The supposed *Credneria* is very like *Populus Leuce*, Ung., of the lower Miocene, and the *Ettinghausiana* seems hardly rightly determined. Besides it is a genus badly founded, and which has as yet no value. All the other plants mentioned by Dr. Newberry belong to genera that are represented in the Tertiary and not in the Cretaceous. And it is very improbable that in America the cretaceous flora has had the characteristic plants of the tertiary; and this would be the case if these plants did belong to the Cretaceous."

It will be seen that Prof. Heer in this paragraph makes several distinct statements, which for the sake of brevity, I will notice in the order in which they occur. They are,

1st. That the fossil plant I supposed to be a *Credneria* is very like *Populus leuce*, Ung.

2d. The *Ettingshausenia* (called erroneously *Ettinghausiana*) is wrongly determined.

3d. That the genus *Ettingshausenia* is badly founded and has no value.

4th. That all the other plants enumerated by me are represented in the Tertiary and not in the Cretaceous.

5th. That it is improbable that in America the Cretaceous flora had the characteristic plants of the Tertiary, as would be

the case if the plants of which outline sketches were sent to Prof. Heer by Mr. Meek, were Cretaceous.

To which I reply,

1st. The plant I considered a *Credneria* is not *Populus luce* Unger, which, according to his descriptions in the *Foss. Flor. v. Sotzka* and *Genera et Species Plant. Foss.* has a toothed margin, while the leaf in question is entire.

I have recently obtained more and better specimens of this fossil, of which I have apparently three species, but as yet have not had opportunity to study them carefully.

They strongly resemble some of the species of that portion of the old genus *Credneria* to which Stiehler left the name (*C. integerrima*, &c.) when he established his genus *Ettingshausenia*. It may prove a new genus. Further study will alone determine this.

2d. That I was wrong in considering some of Dr. Hayden's fossils generically identical with those Stiehler designated by the name *Ettingshausenia*, I am by no means prepared to admit.

Prof. Heer has had but a single outline sketch of the plant, and can hardly speak decisively on the subject. When at Washington I had before me all the figures and descriptions of Stiehler, Zenker, Dunker, Bronn, and Unger, of the genus *Credneria*, and a large number of specimens in good preservation, for comparison. To me, and to Mr. Meek, who examined the subject with me, there seemed to be a marked correspondence in general form, texture and nervation, between our specimens and some lobate *Crednerias* (*Ettingshausenias* Stieh.). With these I regarded our fossils as generically identical, and shall continue so to regard them until the question—if question there be—can be definitely settled by a comparison of specimen with specimen—old world and new.

3d. Whether Stiehler was in error in establishing the genus *Ettingshausenia* upon a group of species of *Credneria*, I will not pretend to say, for I have nothing like the ample material possessed by him when he made the division; indeed this question has nothing to do with that now before us.

It is true that at the Smithsonian Institution I had access to nearly everything that has been published upon the genus *Credneria*, and it seemed to me the most natural thing in the world that Stiehler should give generic value to the cuneate or lobate form, and strongly marked yet finely reticulated nervation, which characterize the group of species of *Credneria* of which *C. cuneifolia* may be taken as a type, while he left the broadly rounded, entire, or merely toothed leaves, with a sparse and rectangular nervation, such as *C. integerrima*—leaves not very unlike those of *Coccoloba*—to which they have been compared—to retain the name of *Credneria*.

The vindication of his accuracy may, doubtless, be safely left to Stiehler. At least it would be nothing short of arrogance for any one who had not before him a suite of the specimens compared by Stiehler, to review his work and pronounce it either erroneous or correct.

4th. The statement that aside from the so-called *Credneria* and *Ettingshausenia*, *all* the genera enumerated in my letter to Messrs. Meek and Hayden, are represented in the Tertiary and not in the Cretaceous, is at least surprising. I am almost inclined to infer from it that Prof. Heer, though confessedly the highest authority in reference to the Tertiary flora of Europe, has neglected to acquaint himself fully with that of the Cretaceous formation. He makes the statement doubtless in good faith, but he can hardly have seen Stiehler's paper on the Cretaceous plants of Blankenburg, and if he has not seen that he is certainly not yet prepared to discuss intelligently the claims of *Ettingshausenia* to be recognized as a good genus; nor indeed the Cretaceous flora in any of its aspects.

Whoever will take the trouble to examine Stiehler's paper (*Palæontographica*, 1857) will see in the enumeration of plants found in the Lower Cretaceous strata (Quader sandstein) *Populus*, *Salix*, *Acer*, and several other genera which Prof. Heer says are represented in the Tertiary but not in the Cretaceous.

The fossil flora of Blankenburg is indeed strikingly like that of our Lower Cretaceous formation, from which the plants that have given rise to this discussion have been derived, except that ours is more varied, and we have as yet found no palms or *Cycadaceæ*.

5th. In regard to the probability or otherwise that the Cretaceous rocks of America should contain a flora similar to that of the Tertiary, it may be said, that it is not now a question of probabilities but of fact, the evidence of the case being now before us, and in abundance.

In what has heretofore been written in reference to these fossil plants two great questions have been raised, 1st, as to their botanical affinities, 2d, as to their geological position.

As to their botanical relations—outline sketches of a few of the plants have been examined by Prof. Heer. By him they were decided to contain representatives of the genera *Liriodendron*, *Populus*, *Laurus*, *Sapotacites*, *Phyllites*, *Leguminosites*, &c., and were pronounced Lower Miocene.

The entire collection was placed in my hands for examination and description before I knew that Prof. Heer had been written to on the subject. I supposed I found among them *Liriodendron*, *Salix*, *Alnus*, *Populus*, *Platanus*, *Pyrus*, &c., with the Cretaceous genera *Credneria* and *Ettingshausenia* and considered them Cretaceous. That you may see on what evidence that opinion was

based, I enclose a copy of my letter to Messrs. Meek and Hayden, which I chance to have with me.

Washington, D. C., Nov, 12th, 1858.

Messrs. MEEK and HAYDEN,

Gents: The fossil plants which you requested me to examine, I have looked over with great pleasure, and, in answer to your question as to the age of the strata from which they were derived, concur with you in the opinion that they belong to the Cretaceous epoch. They include, however, so many highly organized plants, that were there not among them genera exclusively Cretaceous, I should be disposed to refer them to a more recent era.

A single glance is sufficient to satisfy any one that they are not Triassic. Up to the present time no angiosperm dicotyledonous plants have been found in rocks older than the chalk, while of the eighteen species which compose your collection sixteen are of this character.

What was the general aspect of the flora of our Cretaceous continent we can only conjecture, as the specimens of it which we have, represent only its ruder and coarser elements,—the leaves of some of its deciduous trees, which, perhaps by an annual frost, were, as now in autumn, scattered on the surface of stream, lake, or sea, and, sinking, mingled with the sediment accumulating at the bottom.

In such an herbarium we could expect to find little else than the relics of some of the ligneous plants, and a very imperfect picture of the flora of the period.

The evidence furnished by your specimens is, however, good as far as it goes, and we are warranted in inferring from them the existence of a more highly organized flora during the Cretaceous period than has usually been attributed to it.

A flora so highly organized, embracing so many angiosperm dicotyledonous plants, should lead us to expect the discovery of what have not yet been found, plants of this rank in the Jurassic and Triassic rocks. Such a flora as is indicated by your specimens, could hardly have at once burst into being, but was doubtless preceded in the older formations by more or less highly organized plants, the prophetic types of those which followed them.

From the enumeration of the genera represented in your collections it will be seen that the flora of the Cretaceous epoch was not very unlike that of the temperate portions of our continent at the present time. The same thing may be said of the Miocene Tertiary flora of the Upper Missouri so fully illustrated in the collections of Dr. Hayden. In both the tropical and sub-tropical forms so common in the floras of the same period in Europe, are apparently wanting; indicating a greater relative uniformity of climate during the later geological epochs, and carrying the aspects of nature of the present, far back into the past. Thus it may be said of our plants as of our fishes, that many of them are "old-fashioned" types.

An interesting fact in this connection, to which I can only allude, is that the later extinct floras of Europe are more like the existing flora of North America, than is that now growing over the rocks which contain them.*

* Including as they do *Liquidambar*, *Liriodendron*, &c., now exclusively American

The species of your fossil plants are probably all new, though generally closely allied to the Cretaceous species of the Old World. From the limited study I have given them, I have referred them to the following genera:

Sphenopteris,	Cornus,	Salix,
Abietites,	Liriodendron,	Magnolia?
Acer,	Pyrus?	Credneria,
Fagus,	Alnus,	Ettingshausenia.
Populus,		

Of these the last two are exclusively Cretaceous and highly characteristic of that formation in Europe.

For comparison with the preceding list of genera, I subjoin a catalogue of the Cretaceous genera found at Blankenburg in the duchy of Brunswick, given by Stiehler in the *Palæontographica*, Sept. 1857.

Algæ	Credneria,	Pterophyllum,	Comptonites,
	Chondrites,	Flabellaria,	Populus,
	Halymenites,	Pinites,	Alnites,
	Delessertites,	Geinitzia,	Acer,
	Equisetum,	Araucarites,	Quercites,
	Pecopteris,	Salicites,	Juglandites.

I may say, in confirmation of the assertion that your fossil plants are Cretaceous, that I found near the base of the Yellow Sandstone series in New Mexico—called Jurassic by Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours—associated with *Inoceramus*, *Gryphæa*, and *Ammonites*, of Lower Cretaceous species.

Yours, &c.,

J. S. Newberry.

Since that letter was written, I have added largely to my material illustrative of the American Cretaceous fauna and flora, having been for some months engaged in studying that formation over a large area, and where it exhibits an unequalled development.

Of the geological age of the deposits which contain the fossil leaves of which sketches were sent Prof. Heer, there cannot now be the slightest doubt. I have in my hands over sixty species of dicotyledonous plants obtained from the Cretaceous formation. At least half of these are derived from near the base of that system in New Jersey, Nebraska, Eastern, Middle and Western Kansas, New Mexico and Utah, collected by Prof. Cook, Mr. Meek, Dr. Hayden and myself. Some of the species are common to nearly all the exposures of the Lower Cretaceous sandstones, which I have examined, and everywhere serve for the accurate identification of these strata. Overlying the rocks containing all this flora, in the same continuous section, where the strata are conformable and undisturbed, both Dr. Hayden and myself have, in repeated instances, found many of the most characteristic fossils of the chalk, such as *Gryphæa Pitcheri*, *Inoceramus problem-*

aticus, Ostrea congesta, Baculites ovatus, Ammonites placenta, Scaphites Conradi, Ptychodus Whipplei, &c.

The botanical character of this group of plants is, in all essential respects, just what I represented it to be in my letter to Meek and Hayden. Among them are certainly *Populus, Salix, Alnus, Platanus, Liriodendron, Fagus, Quercus*, &c., the most common genera in our present forests.

The plant regarded by Prof. Heer as identical with Unger's *Laurus primigenia* is not a *Laurus*, but a *Salix*, as Prof. Heer would have seen if the specimen had been sent him, instead of an outline sketch. As I have before said, his *Populus leuce?* is not that species. The plants which he calls *Sapotacites* and *Leguminosites* are of doubtful affinity, but certainly not referable to these genera. The latter has a nervation closely allied to that of some of the *Rhamnaceæ*. *Phyllites* is not, as Prof. Herr is made to say in Marcou's pamphlet on "American Geology," "peculiar to the Lower Miocene," but is a general receptacle for fossil leaves of all ages of which the botanical affinities are doubtful, just as *Carpolithes* is a general name for fossil fruits.

It is greatly to be regretted that Prof. Heer could not have applied his great knowledge to the specimens themselves rather than to outline sketches; or, at least, that he should not have been permitted to exercise his excellent judgment unbiased by erroneous oral testimony.

The remarks of Prof. Heer on the fossil plants from the Pacific coast described by Mr. Lesquereux, are exceedingly interesting as forming a new page in the botanical history of American geology, and yet the quite different flora which has come under my observation from the Miocene strata of another part of the continent proves that what he has predicated of the flora, and hence the climate of the continent, though doubtless true of the region where Dr. Evans' fossils were found, is not of universal application.

The study of the floras of the different geological formations has always seemed to me to promise much toward giving us a just idea of the physical geography of our continent, during the different geological epochs. Acting on this conviction in such parts of the continent as I have visited, the fossil plants found, and the nature of the sediments containing them—generally the direct debris of the ancient land—have been to me objects of special interest and attention.

The general results of these observations on the extinct floras of North America may be very briefly stated as follows:

1st. The flora of the Devonian and Carboniferous epochs in America was, in all its general aspects, similar to that of the Old World, which has been so fully described; most of the genera, and a larger number of species than at any subsequent period

having been common to the two sides of the Atlantic. The relative number of identical species has, however, it seems to me, been somewhat overrated. In many of the species regarded as the same in Europe and America, the American plants present prevalent or constant characters which may serve to distinguish them. These differences, though frequently remarked by writers, have not been thought to have a specific value; yet it is quite certain that they are as tangible and important as those which now separate many American and European species of recent plants and recent or fossil animals. I have a conviction that the progress of science will considerably diminish the proportion of identical species; a closer scrutiny and more extensive comparison of specimens resulting in the discovery of constant, though inconspicuous characters which shall be ultimately conceded to be specific.

It is true also that in molluscous palæontology, recent geology and botany, the number of species common to the two continents has been considerably reduced of late years; a large number of American representatives of European species at first considered identical from their striking and obvious coincidences, having, on closer study, afforded constant, though less conspicuous differences.

2d. The Permian, Triassic and Jurassic rocks have hitherto furnished us but few species for comparison, but the material is increasing, and I have now on hand quite a collection which has not yet been studied. Enough is already known to show that the great revolution which took place in Europe at the close of the Permian epoch was matched by a parallel though less sudden change in the flora of America.

Here as there the Lepidodendroid trees, the *Sigillariæ*, the *Nœggerathiæ*, the *Asterophyllitæ*, and the great variety of ferns that gave character to the Carboniferous vegetation, were superseded by *Voltzia*, *Tæniopteris*, *Camptopteris* and a varied and beautiful Cycadaceous flora, in which were many species of *Zamites*, *Pterophyllum*, *Nilssonia*, etc., the representatives of those of the "Age of Gymnosperms," which culminated in the Jurassic epoch of Europe.

During this great interval the generic correspondence between the floras of Europe and America was perhaps as plainly marked as during the Carboniferous age, but the relative number of identical species was apparently smaller.

3d. At the commencement of the Cretaceous epoch the flora of the continent was again revolutionized and the vegetation of its temperate portions given the general aspect that it now presents.

This statement will surprise many, for the flora generally ascribed to the Chalk period is greatly different from that of the

present. Unger has thus represented it, and Brongniart calls it a transition from the great Cycadaceous flora, of the Jurassic period, to the Angiospermous flora of the Tertiary. In Europe the Cretaceous flora was, apparently, more like that of the Lias and Oolite than in this country, for while the genera *Salix*, *Acer*, *Populus*, *Alnus*, *Quercus*, &c., were then introduced there as here, its general aspect was modified by the presence of numbers of *Cycadaceæ*, and its sub-tropical character attested by fan-palms.

We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but as far as at present, known, our plants of this age present an *ensemble* quite different. I have now some sixty to seventy species of Cretaceous plants collected in New Jersey, and in various parts of the great Cretaceous area of the interior of the continent, all of which indicate a flora very similar to that now occupying the same region; many, perhaps most, of the genera being now represented in our forests—such as *Liriodendron*, *Platanus*, *Acer*, *Populus*, *Salix*, *Alnus*, *Fagus*, &c. These specimens have been collected in localities included between the 36th and 41st parallels of latitude, but range from the 74th to the 110th of longitude. Nowhere within this area have I yet detected any traces of palms or any indications of a tropical climate. At the base of the Yellow sandstone series of New Mexico, (Lower Cretaceous,) I have found a varied and interesting flora, containing *Pterophyllum*, *Nilssonia*, *Camptopteris*, &c., with a few Angiosperm dicotyledonous leaves. This is evidently the point of junction between the Cycadaceous flora of the Jurassic age and that of the Chalk; for in the entire overlying Cretaceous strata, 4000 ft. in thickness, though Angiospermous leaves are abundant, those of Gymnospermous plants were nowhere discovered, nor any traces of palms, either leaves or stems. The sandstones of the Cretaceous series contain immense numbers of silicified trunks but they are for the most part coniferous.

4th. For the glimpses I have obtained of the Tertiary flora of North America I am mainly indebted to the kindness of Dr. Hayden, who has spent several years in most successfully exploring the geology, botany and zoology of the country bordering the Upper Missouri. Among his rich collections are fifty or more species of beautifully preserved fossil plants from the Miocene, which have been put in my hands for examination, and of which descriptions will be published immediately after my return to Washington.

Not having the specimens, or my notes on them, with me, I can speak only generally of the flora they represent. I remember, however, that they include species of *Platanus*,—one of which closely resembles Unger's great *P. Hercules* and is perhaps

as large; *Populus*, *Acer*, *Castanea*, *Sapindus*, *Carpinus*, *Ulmus*, *Diospyros*, *Quercus*, *Salix*, *Taxodium*, and others which indicate a flora in all its general aspects similar to that now occupying the Valley of the Mississippi. A few plants in the collection would seem to have required a somewhat warmer climate than that which the localities where they are found enjoy at present; but there are no palms among them, nor any of the tropical genera *Cinamomum*, *Sterculia*, *Dombeyopsis*, &c., so common in the Tertiary strata of Europe.

In the enumeration of the Miocene plants of the Pacific coast, given by Mr. Lesquereux in the May number of this Journal,* I find also evidence of a marked and interesting difference of temperature during the Tertiary epoch, in different parts of the North American continent, under the same parallels of latitude. Mr. Lesquereux finds in Dr. Evans's collection Palms *Salisburia*, *Cinamomum*, &c., which indicate, at least a sub-tropical climate; a flora quite unlike that from the Miocene of the Upper Missouri, although, as he remarks, similar to that of the Miocene of Europe.

I am tempted to dwell for a moment on the interesting glimpses of the physical geography of our continent in geological times, which these facts and others that have come under my observation afford; for, to you, who have done so much toward the elucidation of its geological history, this cannot, I am sure, be a matter of indifference, but my letter has already grown to an unreasonable length. Let me then close with a few generalizations, referring you to my reports for all details of fact sustaining them.

1st. A large continental area occupied the place of the interior of North America from the earliest Palæozoic ages.

2d. During the Carboniferous epoch this land sustained a vegetation similar to that of the Coal period of Europe and Eastern America, though far less varied.

3. Through the Triassic and Jurassic ages the sediments from the land were strikingly like in mineral character to those of the same age in the Old World: and the flora was characterized by a preponderance of Cycadaceous plants, analogous to those of the Jurassic of Europe.

4th. In the Cretaceous age, the central nucleus of the continent was sufficiently extensive to furnish from its ruins arenaceous sediments that now cover more than half a million square miles. These sediments contain vast deposits of carbonaceous matter, mainly derived from the land plants which covered the continent. As far south as lat. 35° these plants were for the most part Coniferous or Angiospermous, and included many genera now characteristic of temperate climates.

Through the Tertiary epoch our continent had nearly the form and area it now has, the Tertiary deposits merely skirting its

* xvii, [2], 361.

borders. The Marine Tertiaries are nearly limited to the shores of the present oceans, while the patches of strata of that age found nearer the centre of the continent are all, so far as I have observed or heard, of fresh water or estuary origin. Between the western base of the Sierra Nevada and the Mississippi there are, I believe no Tertiary beds not of this character, and the larger part of the great central plateau has never been covered with Tertiary or Drift sediments, but has, since the close of the Cretaceous epoch, been as now, dry land.

The facts which I have enumerated seem to indicate that over this ancient land the isothermal lines were curved much as now, and that during the Tertiary ages there was, perhaps, as great a difference between the climate of the Pacific and Atlantic water sheds as exists at present.

ART. XXI.—*Abstract of a Meteorological Journal, kept at Marietta, Ohio:* lat. 39°·25 N. and lon. 4°·28 W. of Washington City; by S. P. HILDRETH, M.D.—For 1859.—[Thirty-third annual report.]

1859. MONTHS.	THERMOMETER.						BAROMETER.		
	Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Inches of rain and melted snow.	Prevailing Winds.	Maximum.	Minimum.	Range.
January,	33·00	63	4	17	14	3·10	W., S.W.	29·85	28·95	0·90
February,	37·77	62	10	10	18	7·20	S.W., & E.	29·60	28·85	0·75
March,	48·48	77	25	12	19	5·08	S.E., & S.W.	29·70	28·80	0·90
April,	52·03	83	28	14	16	6·46	N., & N.W.	29·60	28·85	0·75
May,	67·20	90	46	21	10	1·56	S.W., & N.	29·80	29·10	0·50
June,	67·23	93	33	17	13	4·62	S., W., & E.	29·70	29·20	0·50
July,	74·23	102	49	23	8	1·08	E., S.E., & N.	29·73	29·20	0·53
August,	72·10	95	46	15	16	4·46	S., & N.	29·53	29·30	0·23
September,	63·51	86	46	12	18	4·95	S., & W.	29·70	29·10	0·60
October,	49·12	79	26	17	14	2·79	S.W., & N.W.	29·65	28·85	0·80
November,	45·50	72	20	20	10	2·08	S.S.W., & E.	29·85	29·00	0·85
December,	30·18	71	5	12	19	5·17	N. & W.,	29·75	28·80	0·95
Mean for year,	53·38					Rain 48·55 inches.				

☞ *Remarks on the seasons.*—The mean temperature for the year 1859 is 53·38, which is somewhat above the average for this locality.

The amount of rain and melted snow is 48·55 inches. The average in a series of years, being forty-two inches, falling occasionally to thirty-two inches, and again rising, as in 1858, to near sixty-two inches, so that our climate is quite variable in this respect. The number of cloudy days bear testimony to the humidity of the year.

Winter.—The mean of the winter months is 37°·14, which may be considered very mild for this latitude. The thermometer was at no time as low as zero, so that it did not make ice sufficiently thick for filling ice houses, only two or three inches being the extent of the best, and the main supply for summer use was brought from the heads of the Muskingum river. Steam boats were laid up only a few days, by the floating ice, during the winter. In the eastern states the cold was excessively severe. On the tenth of January, after a great snow storm on the eighth of that month, extending from the borders of Pennsylvania to Maine, the mercury sunk in Salem, Mass., to 23° below zero, in the city of New York to 11° below, and at Ogdensburgh, N. Y., to −38°. The extreme cold on the Atlantic coast was said to be greater than at any time during the last seventy years.

Spring.—The mean for the spring months is 55°·90, an unusual high range, being nearly four degrees above 1858, and more than ten above 1857, that being only 45°·89, so that there is a wide range in the temperature of our springs, which is most strikingly apparent in the blossoming of trees, especially that of the peach, there being a variation of not less than forty days in the opening of the fruit buds of this highly prized tree. The later the bloom is retarded the greater the chance for a crop, but so variable is the climate of southern Ohio, that only one season in three can be counted on for the production of this delicious fruit. The apple crop is rather more certain, and yet nearly every other year is a failure from the blighting effects of late spring frosts. But for this drawback it would be one of the most productive countries in the world in fruit, and the valley of the Ohio as celebrated pomologically as it now is for the growth of Indian corn. The unusual heat of the spring is chiefly attributable to the month of May, which was 67°·20, or six degrees above the average temperature, which is sixty-one degrees. The heat was nearly that of June, and rarely experienced, as there is commonly a difference of ten or twelve degrees in these two months. The peach was in blossom this year on the 28th of March, and the apple on the 12th of April. In 1857 the peach opened on the 2nd of May, and the apple on the 9th.

Summer.—The mean of the summer months is 71°·19, which is not much below the average, notwithstanding the uncommonly low temperature of June. The unprecedented occurrence of a severe and destructive frost as late as the fifth of June overwhelmed the country with fear and astonishment; at a period in the growth of wheat usually considered as past all danger of this kind, a sudden change of temperature in one night spread destruction and ruin to a large portion of the fields of this important cereal over all the central portions of the valley of the

Ohio, and extending from Iowa to northern New York. Indian corn shared largely in this calamity. The warmth of May had hastened the growth of this plant in many fields to the height of twelve or eighteen inches. In all such cases the fields had to be replanted, but where it was only a few inches above the surface it recovered from the injury, and produced a fair crop. The wheat being in full head, and much of the grain in the milk, was entirely ruined. Potatoes were badly frosted, but in a good measure regained a healthy state. Peaches and apples, which in most orchards had attained the size of almonds, were so much damaged as to fall from the trees in a few days, and only certain favored localities ripened any fruit. So serious an injury from untimely frost has not been experienced since the first settlement of the state in 1788. In the year 1834, severe frosts visited Ohio as late as the middle of May, but the wheat crop was not so far advanced, being only in blossom, and by throwing up new stalks from the uninjured roots, produced finally an abundant harvest. Providentially the autumnal frosts of 1859 were retarded until near the close of October, and the late planted fields of corn were fully matured, to the great delight and wonder of the husbandman, for the failure of this grain would be a more serious calamity than that of wheat, as both man and beast largely depend on it for sustenance.

Autumn.—The mean for the autumnal months is 52°·71, which is rather below the average, but was sufficient to ripen all the late crops. Sweet potatoes were uncommonly good in quality, and abundant in quantity. Buckwheat was largely cultivated, partly in place of the common grain, and produced a great yield. The Catawba and other grapes ripened well, and abounded in saccharine principle, so necessary in making good wine. The smaller fruits were plentiful, so that on the whole we have more cause to be thankful, rather than to complain of the dealings of Providence in the past year.

Floral Calendar, &c.—February 25th, Bluebird heard; 27th, Yellow garden crocus in bloom; March 4th, Many birds of passage seen and heard; 9th, White crocus; 12th, Hepatica triloba; 14th, Golden bell or Forsythia viridis, Acer rubrum, (Red maple), Ulmus Americana; 16th, Hepatica acutifolia; 21st, Grass quite green in pastures; 22d, Magnolia conspicua, Claytonia Virg.; 26th, Red cherry, Balm of Gilead, and Sugar maple; 27th, Crown Imperial; 28th, Peach tree, Red Pyrus Japonica, Hyacinths; 29th, Sanguinaria Canadensis; 31st, Gooseberry.—April 1st, June berry; 2d, Dandelion, Pink colored Pyrus-Japonica, Cherry and Plum, Primroses; 4th, Flowering almond, Anemone nemorosa; 5th, Phlox divaricata, Dielytra cucul.; 7th, Annona triloba, Papaw; 11th, Burgundy pear, Trillium grandiflorum; 12th, Double flow.-Peach, Siberian crab, Spiræa prunifolia, Cercis Canadensis, or Red bud; 13th, Apple tree;

14th, Chickasaw plum, Strawberry, Sedum ternatum; 17th, White ash tree; 22d, Cornus Florida; 24th, Dodecatheon Amer.; 27th, Harebell; 28th, Vernal snow drop; 30th, Tulips.—May 2d, Tree peony, var. papaw; 4th, Haw tree, Dicentra spectabilis; 5th, Mountain ash, Aquilegia Canad.; 6th, Magnolia tripetala, Viburnum, (Snow ball); 8th, European Horse chestnut; 10th, Rose colored peony, Yellow Harrison rose; 12th, Blackberry and Robinia Pseudacacia; 15th, Rose Acacia and Annual roses; 16th, Iris Persica, Crimson peony; 17th, Purple peony; 18th, Moss rose; 19th, White peony, also several new peonies from seed planted five years ago, bloom first time; 21st, Peas on table, planted in January; 22d, Syringa Philadelphica, Strawberry ripe; 24th, Bulbous Iris; 26th, Foliage of trees unusually rich and fine; 27th, Fragrant peony, and large rose colored; 28th, Linocera flexuosa; 30th, Erigeron annuum.—June 1, Star of Bethlehem; 2d, Common cherry, ripe; 5th, Severe frost, killing wheat, corn and fruit, made ice in a bowl of water, half an inch thick, a few miles west of Marietta; 12th, Canterbury bell in bloom; 19th, Red raspberry ripe; 23d, Pennsylvania lily in bloom; 27th, Wheat harvest begins in fields that escaped the frost, on high hills, or where protected by the fog from the rivers; 28th, June apple ripe.—July 5th, Chestnut tree in bloom; 8th, Sweet bough apple and Gravenstein ripe; 12th, Gladiolus flori; 13th, Blackberry ripe, but a large portion destroyed by frost, American broom in bloom.—August 8th, Catherine pear ripe; 13th, Watermelon ripe.—September 1, St. Michael pear; 15th, Seckel pear fully ripe.

In every month during the past year there has been more or less frost, as in the year 1816.

Art. XXII.—*Geographical Notices;* by Daniel C. Gilman. No. XI.

Biographical Sketch of Dr. Karl Ritter.—The death of Dr. Karl Ritter, the father of the modern science of Physical Geography, and one of the most eminent and beloved of the scientific men of Germany, has already been announced in this Journal. We present herewith a sketch of his life, translated and condensed from a highly interesting tribute to his memory which is attributed to the pen of Dr. Kramer of Halle, in the Berlin *Zeitschr. für allgemeine Erdkunde.*

Charles Ritter was born at Quedlinburg the 7th day of August, 1779. His father, a man of noble character, fine feelings and a pious mind, was physician to the Abbess of the Convent there, and was much esteemed for his skill. However, in consequence of the slanders of an envious individual, he lost the

largest part of his practice, and, although his good name was restored after the lapse of two years, and his clients returned, grief and sorrow had so heavily weighed upon him during this time, that in the full strength of manhood, he succumbed to a typhous fever. He left an almost destitute widow with five little children, of whom the eldest, a boy, was ten years of age, the fourth, Charles, only five years old. This situation of the poor widow, a noble and highly educated woman, excited the utmost sympathy of her neighbors. All endeavored, either by words, or in a more substantial way, to make good the wrong which had been done to her husband. She found many sympathizers, away from home. Thus, the Prince of the adjoining Bernburg took care of the education of the eldest boy. Salzmann, the celebrated educator, a former associate of Basedow, had bought Schnepfenthal, and was about to open an educational institution there. He had made it a point to take a boy as his first pupil, gratis. A notice in a journal of the death of Dr. Ritter at Quedlinburg, who had left a widow with five little children, first attracted his attention. Soon after he sent two of his friends there, to make the acquaintance of the children and to see whether there was a boy amongst them, that would conform to his wishes. They decided in favor of little Charles. The mother, though with a sorry heart, assented, and, at the invitation of Salzmann, brought the child herself to Schnepfenthal. She was accompanied by one of her elder sons and Gutsmuths, then a candidate of theology and instructor of the children, who had not left them, although the mother had declared that she was no longer able to pay him his salary. A residence of a few days in Salzmann's house cemented the ties of mutual friendship and esteem, so that Salzmann, shortly before their departure, expressed a wish to keep the older boy also. To Gutsmuths he proposed to remain in Schnepfenthal as a teacher. This had been a secret wish of the mother, but she did not think it possible. Ritter accordingly came to Schnepfenthal, the first pupil of the new institution, and remained there for eleven years, until he went to the university. This lovely spot, which Ritter always considered his true home, was situated at one end of the 'Thueringer Wald' and was surrounded by a most charming landscape, having in one direction a view of a far extending fertile plain, richly adorned with cities and villages; in the other, there rose well timbered mountains of various shapes, intersected by fine valleys. All around was activity and life. Here he received from his early youth the most vivid impression of the glory of God's creation, of the variety of formations on the surface of the earth, and their special relations to the life upon them. Here Ritter grew up under the guidance of excellent men and skillful teachers. Those that ex-

erted the greatest influence upon him, were Salzmann himself, Bechstein and Gutsmuths, the latter of whom continued here also, to take special care of young Charles, and probably implanted in him a love for geographical knowledge.

The method of instruction was that suggested by Basedow, and tried first in the Philanthropin at Dessau, but it was freed from those vain and needless peculiarities that adhered to it there. Classical languages were less studied, but the most attention was paid to all those sciences and accomplishments, which stand in direct relation to life, and among these the modern languages occupied a more prominent place, than anywhere else. To this an unusual impetus was given by scholars from different countries, who thronged, soon after the opening of the institution, to Schnepfenthal. By physical training, and by strengthening the character and intellect, a general and equal development of body and mind were especially aimed at, and, although practical rationalism pervaded the whole institution, darkening a little the deepest sources of true blessings, there still reigned piety, love, and the purity of high moral sense. Under these influences all those noble qualities of Ritter's heart and mind were developed, that distinguished him so much in after days. The future lay dark before him, and he had not decided upon his course in life, but he felt a strong desire to study, of which however there was as yet no prospect. His mother, though married again several years after the death of her first husband to the celebrated pedagogue Zerrenner, was not able to provide for him. But Providence interposed here also. A rich merchant from Frankfurt on the Main, associated with the large firm of Bethmann, Mr. Hollweg, visited the institution at Schnepfenthal and became very much interested in the young Ritter. After hearing of his circumstances, he declared himself willing, on the recommendation of Salzmann, to furnish the necessary means of study, upon the condition however, that Ritter, after the completion of his studies, should enter Hollweg's house as instructor of his children.

So Ritter went, at the age of 17 years, to the University of Halle, and was matriculated November 2, 1796, as *studiosus cameralium* under the prorectorate of Curt Sprengel. Here he remained for two years. Halle was then the centre of great scientific activity. F. A. Wolf especially was then in the height of his renown. Ritter did not pursue a specific course of studies, as his previous education had not been directed towards that channel, which however he sometimes regretted in later life. He often mentions A. H. Niemeyer, to whose circles he had access, and in whose house he lived, and who exercised upon him an important influence in improving his mind and inciting him to farther study. Niemeyer occupied then a prominent place

in the pedagogic world, and that work by which he became most known, "Grundzuege der Erziehung und des Unterrichts" (or, Principles of education and instruction), first appeared in 1796, and must have been of special interest to Ritter, as he was himself preparing for the calling of an instructor.

In 1798 Ritter left Halle and entered Hollweg's house as instructor of his four children, especially of the two boys, one six, the other three years old. It was a great change for the young man of 19 years, to step out from the quiet circles in which he formerly had moved, into the midst of a world quite unknown to him, and to move among the aristocracy of a mercantile city. He had to struggle with many difficulties. But he went to his task in all earnestness, and with the ardor of a true and powerful mind, conquering all impediments so completely, that he gained results such as but few instructors can boast of. This was especially true in the case of his younger pupil, the other having died in the bloom of his youth. Ritter conducted the education of the former until he went to the University, and this pupil is the present minister of clerical, educational and medicinal affairs in Prussia, von Bethmann-Hollweg. Equally successful was he in the education of a son of the celebrated S. Th. Soemmering, and out of this relation of teacher and pupil grew the most intimate friendship and love, which lasted for life. During his stay in Hollweg's house Ritter came in contact, and even into nearer relations, with many eminent men, and by the intercourse with them his ideas expanded and became freer and more independent. Amongst those that exerted the greatest influence in this respect, S. Th. Soemmering must be mentioned above all, a man of great genius and deep scientific knowledge. Ritter thus speaks of him in the introduction to the second edition of his 'Erdkunde :' 'If in the explanation of the laws of the geographical relationship of all animated nature there should be prominent some interesting opinion and view, then the author is indebted for this whole tendency of investigation to the long, instructive, and I say it with pride, familiar intercourse with a noble man, S. Th. Soemmering, a man who is an honor to his century and nation; for his spirit filled others also with the premonition of the depth of nature, which his own genius had penetrated into its most hidden mysteries.' Ritter was also befriended at Frankfort by J. G. Ebel, the author of the classical work on Switzerland. This was not only of the highest importance for him during his repeated travels to Switzerland in relation to the knowledge of this country, but it also impelled him to farther study. Ritter, speaking of Ebel, says in the above-mentioned introduction: 'What the present work may have of vivacity and warmth, it owes to an intercourse of many years with this excellent man at the time I commenced it.'

Ritter's mind moreover was much aroused by the daily intercourse with men of equal aim, and inspired with the same ardor for the education of youth, such as E. Mieg and J. B. Engelmann. Besides he sometimes came into transient but important intercourse with men of eminence who travelled through Frankfort. He met (to mention only a few names) Alexander von Humboldt and Leopold von Buch in Hollweg's house. But even life itself in this old interesting city, uniting so many instructive elements, showing so many different relations of a most various character, and being situated in the midst of the district of the largest river in Germany, always invited to new observations, wanderings and study.

Ritter used with the greatest ardor all these opportunities to acquire information. The time of his sojourn at Frankfort was a time of the most various studies. So he applied himself with much zeal to the classics, and read with the assistance of his friends, F. C. Matthiae and J. F. Grotefend, then at the head of the gymnasium at Frankfort, the most important works of the Greeks and Romans, but the tendency of his mind towards geography always appeared with marked prominence. In order to become entirely at home in this department, he not only studied thoroughly the most important works on the subject, but he also made observations of his own in frequent excursions to different parts of the country. The ability to draw with the greatest ease those objects in a landscape which were important to him, and so to fix them forever, was of much service to him. He always brought a number of characteristic sketches home from his journeys, which served both for himself and others as illustrations of his observations. This tendency towards geography was manifested in his first contributions to the 'Neuen Kinderfreund,' edited from 1803–1806 by Engelmann in connection with his pedagogic friends, but it became yet more apparent, when in 1806 he published his six charts of Europe, and not long after, in 1811, when his geography of Europe (2 vols.) appeared. In both works the peculiarity of his geographical perception is thus early indicated. They are the groping essays, the *incunabula* of what was lying in his mind. But, before his ideas could come to maturity and light, other preparations were to be made. As such, in different respects, must be considered the journeys which he, from the year 1807, repeatedly undertook with his pupils to Switzerland and Italy, and the last of which, commenced in 1811, comprised several years. These travels must indeed have been a rich source of instruction to his observing and powerful mind, that was so well prepared and matured by assiduous study and labor. Just these countries are the most expressive representations of the most important and most various geographical types which Europe has to show.

Switzerland, the most important parts of which he crossed in different directions, impressed him deeply with the grandeur and glory of a majestic and infinitely rich nature, which invited irresistibly to the study of this gigantic structure. Italy, on the other hand, which he passed through down to its southern point towards Sicily, furnished him important information in reference to volcanic activity, exhibited to him the relation of land to sea, showed the effects of climatic differences and the close connection of the nature of the country with the development of its people. Those treasures of art, which Italy possesses so abundantly above all other countries, must have added their proper share also to his store of knowledge laid up in his naturally fine and carefully educated mind.

During his travels in Switzerland he met many men of eminence and note; amongst them Pestalozzi, von Tuerk, Niederer and many others of that district. He spent many happy and instructive hours in their society, and between him and many of them a mutual friendship was established. He always remembered Pestalozzi, whom he often visited at Iferten, with reverence and gratitude, and had a picture of him in full length in his study.

The most important point in Switzerland for him was Geneva, where he remained from the middle of the year 1811 for more than twelve months. This city was then particularly noted for the active part it took in the cultivation of science, especially natural science, and was much distinguished by the fine tone of its society. Saussure, the first man of the city and the state, had shortly before died, and his pupils, men of European fame, such as M. A. Pictet, de Candolle and others, were considered the centres of the higher circles. With the first, Ritter became very intimate and to him he owes many valuable hints. St. Gervais, close at the foot of Mont Blanc, was another very interesting point for Ritter, as it offered to him an opportunity of observing the nature of high mountains in all their details. From this spot he made that tour around the Mont Blanc, of which he gives such an interesting and instructive description, in explanation of the bas-relief executed by Kummer.

During his travels in Italy, Rome most especially attracted his attention. It was not only the centre of numerous monuments of history and art, but he also met here men like Thorwaldsen, Overbeck, Cornelius, and others, who by their genius and love for the arts, had raised them again to a most flourishing state. By intercourse with such men, Ritter's insight into the nature of Art was much enlarged.

Thus variously enriched, he returned home, and soon commenced that work, which was the chief production of his whole life, and which will make his memory immortal. In order to

prepare it, he went in 1814 to Goettingen with his two pupils, who at that time were beginning their academical studies. Here he used all the means of learning within his reach, he searched out the rich treasures of the library, was in active intercourse with the masters of science, (Hausmann especially was dear to him,) and did not disdain to enter again as a student the auditoriums of the professors and to hear lectures on the most various subjects. After a residence of two years he went to Berlin (1816) and there gave his work its last finish, after which it was put to press. Next year he went again to Goettingen to superintend the publication of his work, of which the first part in 1817 with the title, 'Erdkunde im Verhaeltniss zur Natur und Geschichte des Menschen oder allgemeine vergleichende Geographie als sichere Grundlage des Studiums und des Unterrichts in physikalischen und historischen Wissenschaften,' or, Geography in relation to the nature and history of man, or general comparative geography as a safe foundation in studying and teaching physical and historical sciences. In this work geography had been entirely remodelled and changed. It had indeed been raised to the rank of a true science, constituting the link between the natural sciences and history. The first part contained Africa and part of Asia; a year after, the second part appeared, which completed Asia.

We abstain from giving a detailed description of this work, as it is well known, and as the method, in which geographical matter is treated here, has been adopted throughout the whole scientific world. Ritter's aim is briefly indicated by its title; a more detailed account of its leading ideas however is given by himself in his introduction to the second edition of the first volume, which appeared in 1822. Ritter's intention was, to give with the greatest accuracy a vivid image of the formation of the superficies of the earth in its horizontal and vertical dimensions by means of a conscientious and careful use of all existing sources, and to represent and explain the characteristic qualities of its parts and their relation to each other and to the whole earth, but at the same time to make it serve as a substratum of all animated nature, and as a foundation and condition of the development of the different nations and the whole human species in their manifold mutual relations to one another.

This was a stupendous task, but Ritter performed it marvellously well. Its execution required a combination of great and varied talents, such as rarely ever have been or will be found, cultivated by deep and assiduous study. In it we see powerful and truly ingenious displays of general geographical intuition and combination, we perceive a care, that indefatigably penetrates into the deepest recesses and most minute details, we find evinced an extensive knowledge of the natural sciences

and a perfect command of extensive historical materials, and lastly, a truthfulness and thoroughness of learned inquiry combined with the rare gift of a rich, fresh, vivid and expressive representation. Truth and knowledge of the living God were the springs that actuated his mind and after which he aspired. Hence his humility, hence his close and perfect application, his concentration upon the subject before him. No difficulty ever deterred him in his investigations, although the matter before him was continually and vastly accumulating. His work was to him, as he wrote in his diary, when, after a long interruption, he again commenced his labor, 'his song of praise to the Lord.'

When Ritter had completed the first two volumes of his geography (Erdkunde), in connection with a work that was the direct result of his Asiatic studies, 'Vorhalle Europaeischer Voelkergeschichten vor Herodotus um den Kaukasus und an den Gestaden des Pontus,' or, 'Vestibule of the history of European nations before Herodotus around the Caucasus and at the shores of the Pontus,' he received in the year 1819 a call to be Professor of history at the Gymnasium of Frankfort. In the fall of the same year he married. In September 1820 he accepted a call as Professor extraordinarius of history in the Military School and the University of Berlin.

At Berlin the second half of his life commences, and here the richest fruits of his former labors and preparations matured. Berlin was indeed the most favorable city for such an object as he had in view. Nowhere were the means for study and instruction so abundantly and generally supplied. Both the University and the Military School displayed great scientific activity, which was kept alive by eminent scientific men, into whose circle Ritter soon entered as a highly esteemed member. His lectures were soon well attended at both institutions. Besides he kept up an active intercourse with other scientific celebrities, among whom Leopold von Buch, but especially Alexander von Humboldt may be mentioned, whom he highly esteemed, and with whom he lived on terms of the most intimate friendship. Moreover, Ritter lived here in the midst of his nearest relations; one brother lived in Berlin, another near the city, and among his dearest friends, was his former pupil, Hollweg, then also professor at the University. All this encouraged him much, and instigated him in various ways to still farther study and advancement. Ritter's activity was now, besides the duties of his office, chiefly directed towards preparing a second edition of his Erdkunde, of which the first volume appeared in the year 1822. It was much enlarged and in every respect more complete than in the first edition, although now only comprising Africa. The continuation of this work however suffered a long interruption

and the chief reason for it was, that his official duties claimed his attention more and more. Accordingly he entered, though only for a short time, as a member, the scientific examining commission for history and geography, and soon after the death of his friend Woltmann he also undertook the historical lectures at the Military School.

In 1825 he was appointed Director of Studies to the corps of cadets. Besides, he instructed Prince Albrecht of Prussia in history for many years, and received, especially during the winter months, frequent invitations from the Crown-Prince to lecture on history and geography before him and some of his nearest relatives and friends. Similar invitations came also from other sides, and he very often complied with them. Thereby a very considerable part of his time and strength was claimed, as he was always wont to apply the utmost care and scrupulousness to all his subjects. Nevertheless by his extraordinary diligence, which was a necessity to him, assisted by a robust healthy body and by his collectedness and freshness of mind, he still found leisure to work and to promote and advance his scientific labors, which were never lost sight of. The external fruits of these labors were however confined in that period, to his reports in the Academy of Science, of which he was a member after 1822, and to brief essays, like the one on India in the Berlin Almanac for 1824. Many of the results of his studies were communicated to the geographical society, which he, in 1828, had founded in connection with several friends, and of which he was the chief supporter. Those travels which he regularly undertook during the long fall vacations were of the highest importance for him in every respect. They not only served him for bodily and mental recreation, but were also very useful for the advancement of his geographical studies, whether he was occupied in the observation of nature itself, or by investigations in important geographical centres, as Vienna, Paris, London, and other places. These journeys extended in very different directions over the countries of Central Europe and sometimes occupied the larger part of summer. The most extensive and important were: a tour to Greece, Constantinople, through Bulgaria, Wallachia, Sieben-Burgen and Hungary; repeated travels to Paris, through the southern, and at another time, western part of France, and the Pyrenees; through Belgium and Holland; through Denmark, Sweden and Norway; to London, and through a part of England. He often visited and explored, but always in different directions and with different objects, the middle and southern part of Germany, the system of the Alps in its different parts, Switzerland, and the northern part of Italy. The results of these journeys were a great variety of impressions and observations (mostly recorded and narrated in his detailed

and highly interesting letters to his family), oral communications of a most varied character, and the establishment of manifold personal relations and connections.

In the year 1831, Ritter withdrew from all business and labor foreign to his geographical studies. He felt that if he would advance geographical science, which he considered the task of his life, he must concentrate his powers. Thus, when he again had leisure, the fruits of his studies became more apparent. There now appeared, from the year 1832, in quick succession, that series of volumes on Asia, of which he concluded the nineteenth a few weeks before his death. This work will be a lasting monument of his genius, and a standard work for all ages, however great the progress of geographical science may be hereafter.

The author's name grew in proportion with the progress of the work, his acquaintances increased in all civilized countries of the world, and his influence upon the course of geographical investigation and science was greatly augmented. He became one of the most important personal centres for the science, since he possessed an immense store of knowledge, and a sound judgment, and took a most active interest in all questions relating to this subject. He entered into everything, even if trifling or troublesome, with an amiability and urbanity that never tired.

He received marks of acknowledgment and distinction of all kinds. Most of the learned societies, in and out of Europe, made him a member, many Orders were given to him, and his Sovereign gave him frequent proofs of his personal favor during the many years of his residence at Berlin. Ritter occupied undoubtedly, as savant and author, one of the most exalted positions among his contemporaries. But he was not less great as a teacher also. There were few lecturers who exercised such an invariable power of attraction as he did. When in 1820 he first announced his lectures on general geography, no hearers came forward, and in the course of the term only a few presented themselves. Ritter began his lectures, but even in the next semester hearers were yet scarce, and this must not be wondered at, as but a few of the students had heard anything of Ritter, and the great majority of them considered geography as something hardly worth the hearing. This state however soon changed, and already in 1823 Ritter wrote in his diary: "Full auditory, I must take a larger one." The numbers of his hearers increased from year to year, so that sometimes even the largest auditory scarcely could hold them. Ritter was now looked upon as the one whose lectures had to be attended by every student of a high scientific aim. Which of his numerous hearers does not remember with gratitude the pleasant and instructive hours of his lectures? Ritter showed a most perfect

tact, acquired by many years' experience; he knew how and what to select from the immense store of matter over which he had a perfect command; he knew what was best adapted to a verbal exposition and from what the greatest benefit would be derived. His delivery showed that he had deeply and thoroughly mastered his subject, which he always elucidated by drawings, placed on the black-board with great ease. Every one of his hearers felt the importance of the subject, perceived Ritter's deep scientific researches, and was delighted at the results, which were made so accessible, and at the improvement gained by means of them. His lectures were always instructive and always excited to farther study. His delivery was dignified, but everywhere and under all circumstances unassuming. His purity of mind, his modesty and amiability shone forth everywhere, and exercised a peculiar charm especially on those that came in closer contact with him. None ever approached him without meeting a most friendly and hearty reception. No effort in science, however imperfect, was made, that he did not acknowledge and encourage by his counsel and assistance. Egotism was entirely foreign to him; he was the truest and most affectionate friend; in his family most tender-hearted and loving, and his greatest pleasure was to see those around him happy. He himself was without children, but he was a father to many that were comparatively strangers to him. His mildness of temper exercised a most soothing influence upon all; his peace of mind, pervading his whole nature, could not easily be shaken, even by severe losses, such as the death of a dear sister and of his beloved wife in her full strength, both occurring in the course of a few days. These noble qualities of mind were the precious fruits of a strong and living faith. Ritter was a Christian in the full meaning of the word, although never saying very much on the subject and never raising himself up as judge of the religious belief of others. The holy word of God accompanied him everywhere, and to confirm its truth by the results of his investigations was his highest joy. His own words found after his death, are the best testimony: 'Although at present, while preparing for a journey to the western part of France and the Pyrenees, I am healthy and well, life nevertheless lies in the hand of God, whose mercy and grace has guided my fate so wonderfully and gloriously, that I cannot but sing to him, the Allgood, praise and glory with all my power, in all my thoughts and actions. Should it not please him to let me return to my beloved family and to my calling, but should he assign to me another place in his heavenly kingdom, that I may obtain happiness, a happiness which already here sometimes has moved me to tears of joy, then I ask my friends not to grieve over my going home, for all that the Lord does is done well.

My eternal fate my Savior in his great mercy will decide. In deep acknowledgment of my infirmities and sins, I am still full of trust and confidence, since I know that my Redeemer lives, who will make his people partakers of the mercy of the Eternal and Just one.'

Ritter's health was generally good. His constitution was strong and was hardened by exercise. His numerous travels, often on foot, renewed his strength, when weakened by close application to his fatiguing scientific labors. In his last years, however, many infirmities were felt. The Teplitz medicinal springs had relieved him several times, and Ritter tried them again in July 1859, but this time without relief. Great heat and hot baths seem to have weakened him more, and this weakness was still more increased by frequent hemorrhages from the bladder; he lost his appetite, and his strength begun rapidly to fail, even when his appetite partially returned. He died September 28, 1859, at 10 o'clock A. M., and was interred October 1, in the Marienkirchhof by the side of his beloved wife, who died in 1840.

MR. LENTZ'S REPORT OF HIS EXPLORATIONS IN PERSIA AND AFGHANISTAN.—We have received through the Smithsonian Institution, Washington, reports of the meetings of the Imperial Geographical Society of St. Petersburg, Oct. 7, and Nov. 4, 1859. From the former of these we translate the following account of the Russian expedition under Messrs. Khanikoff and Lentz into Afghanistan and Persia.

Mr. R. Lentz, who took part in the expedition to Khorassan, presented some interesting information in regard to the scientific results of his travels during the sixteen months which he passed in Persia and Afghanistan.

The main object of Mr. Lentz in his travels was to determine the geographical position and elevation of the places, which he visited; to investigate the three elements of terrestrial magnetism (declination, inclination and force of tension); to ascertain the heights of the mountains; and finally to make meteorological observations.

The expedition arrived at Astrabad, in the province of the same name, early in April 1858. As Mr. Khanikoff, to whom the direction of this expedition was confided, had gone to Teheran, Mr. Lentz employed his time, most profitably, during the absence of the former, in observing the movement of the chronometers and in ascertaining the absolute longitude of the city of Astrabad. He also determined at this place, in the same manner as at Ziared, the three magnetic elements, ascertained the height of some of the summits in the Albourz chain, and made meteorological observations.

Towards the middle of May, the expedition left Astrabad, and, having passed Albourz, stopped at Schakhroud. Mr. Lentz observed here, as everywhere in his travels, the movement of the chronometers, and determined again the three magnetic elements and the latitude of a great number of peaks in the Albourz chain.

On the first of June, when Mr. Khanikoff had returned, the expedition directed its course towards Meschel, the actual capital of Khorassan. During the whole route Mr. Lentz took great pains to determine the greatest possible number of geographical points and ascertain the elements of terrestrial magnetism. The five weeks of his sojourn at Mesched were spent in works of this kind, and he also made an excursion in the neighborhood in order to effect astronomical and barometrical observations.

The expedition left Mesched in the first part of August and reached Herat during the first days of September; having already determined a considerable number of geographical points, the position of which had been previously unknown.

In Khorassan, Mr. Lentz was chiefly occupied in determining with the greatest possible accuracy the absolute longitude of the most eastern point touched by the expedition. From Herat Mr. Lentz advanced to Tebbes, which is situated at the eastern limit of the salt desert of Khorassan, and then to Birdia-Sand, and succeeded in collecting many valuable additions to science.

In the midst of February 1859, the expedition left Khorassan and took its course towards Lasch, a fortified city and the capital of a little state of the same name. The observations made in this country are particularly interesting. They show that the terrestrial surface rises gradually from Herat to the passage Senhighé-Sia, near the city of Sabzor (Kingdom Hérat), where this elevation reaches a height of 5000 English feet; from this point the country gradually descends to a lake, the waters of which, however, have still an elevation of 1200 feet.

The expedition stopped, in its course, on the eastern shores of lake Zaré, near the place, where the river Kharoud or Adraskan empties into it. Mr. Lentz determined the absolute height and the geographical position of this point. The appearance and the dimensions of the lake change continually, sometimes the northern portion of it is dried up, and only its southern part is seen; sometimes the reverse takes place, according to the quantity of water, furnished by the three principal rivers, which empty into it, viz.: the Kharoud and the Ferraroud in the north, and the Khilmend, which flows not far from Kandahar, in the south. Sometimes it happens, that the waters of the lake divide into two parts, one towards the north, and the other towards the south, and are kept separated by a band of completely dry land as was the case at the time of the expedition. This at least is the ac-

count given by some Persian travellers, who had recently left the eastern shore of the lake and declared that they had crossed it by following this tongue of land.

Beyond the village of Nekh, the expedition came to a desert of 250 versts in extent. This they crossed in its narrowest part between the villages of Serri Tschakh and Dekhi-Seïf, at a place where in a length of 200 versts no trace of water was found. After a journey of four days they reached Kirman.

This place might be considered as entirely unknown up to this time. It appears from barometrical observations made there, that from lake Zaré (1200 feet high) the surface of the earth rises again up to the villages of Nekh and Serri-Tschakh (4000 and 5000 feet high), when the country gradually descends to its lowest point (900 feet high) in the desert at a place called Schakhri-Lut, but rises again as far as Khubbis, which is situated at the foot of the mountains, at an elevation of 1500 feet, and reaches its maximum (8000 to 9000 feet) at the top of these mountains, then it falls again towards Kirman, which however is still found to be 5500 feet high.

Over the whole area, which extends from Esd to Ispahan, (the point toward which the expedition directed its course,) the countries which border south and east upon the great salt desert, are 3000 to 4000 feet above the level of the sea; the same is the case with those countries which separate Schakroud from Mesched along the northern side of the desert; only in two places more considerable heights are found.

From Ispahan the expedition passed through Zerghendé, 10 versts from Tehran, went beyond the village of Firouska, reached the provinces of Mazanderan and Astrabad, and followed the course of the river Talar, which empties into the Caspian Sea.

Mr. Lentz determined, during his travels in Persia and Afghanistan, about two hundred geographical points; at twenty-eight points he could determine the three magnetic elements (intensity, declination and inclination); at twenty-nine other points, however, he observed only two of them. He ascertained with the aid of the trigonometrical calculus, the heights of about two hundred mountain summits, and about four hundred others were measured by him, and his travelling companions, with the aid of the barometer. Thus Mr. Lentz traced a profile of the whole country, which the expedition visited. Mr. Lentz gives also some interesting results of his meteorological observations, which he never neglected during the whole voyage. The barometrical observations show, with an incredible regularity, the slightest variations of the atmospheric pressure; the variations of temperature are also very regular, and it is interesting to state, that the temperature reached constantly its maximum about four hours after noon, and not two hours, as it is generally the case in our climate.

Throughout Khorassan the air is usually very little charged with vapors and its average hygrometrical state varies between 20 and 30 per cent. At Schakhroud, Mr. Lentz found only 17 per cent of moisture in the atmosphere, and in the desert near Kirman, only 14 per cent."

SCHLAGINTWEIT'S ETHNOGRAPHICAL COLLECTIONS.—Mr. Joh. Ambr. Barth, of Leipsic, has offered for sale a large collection of plaster-casts taken from the heads, hands and feet of individuals in the different castes and tribes of India, and has published a carefully prepared catalogue of the series. We make the following extract from his announcement:

"Messrs. Hermann, Adolphe and Robert de Schlagintweit, the enterprising travelers in India and High-Asia, having, since the year 1854, had charge of a scientific mission from the India House, have been enabled during their travels, in addition to their researches in physical geography and geology, to devote much of their time to ethnology.

"The various countries through which they passed, some of which have hitherto been but little explored, and others never reached by Europeans, afforded peculiarly advantageous opportunities for pursuing their ethnological researches.

"Besides measurements and photographs, they, in collecting their materials, made also casts of the features of living persons taken in plaster of Paris; 275 casts of faces were thus made, and 37 of hands and feet.

"The moulds have been reproduced by galvanoplastic deposits of copper, which gives without the least contraction the most minute irregularities of the skin with great perfection. This first series however was found not sufficiently strong, and the attempts which have been made to produce the heads in as great perfection as possible, have led to a different method, consisting in making strong metallic casts of zinc the basis, coated with a galvanoplastic deposit of copper, varied in color according to the different degrees of color of the native tribes. To exclude as perfectly as possible the change of the tints by the gradual oxydation of the copper, a thin stratum of colorless varnish has been put most carefully over the heads.

"Together with these casts, measurements of the various proportions of the skull and the body have been taken, which will be spoken of in detail in the work, which Messrs. de Schlagintweit are about to publish under the auspices of the President and Council, now at the head of Indian affairs, who having the same interest in science as the Court which preceded them, have found this collection an object of particular importance.

"This work, 'Results of a scientific Mission to India and High Asia,' is independent of this collection: it is in progress of being printed and is published by F. A. Brockhaus, publisher at Leipzig.

"The ethnographical part of Messrs. de Schlagintweits' work will chiefly treat of characteristic features obtained by well defined measurements, constantly referring to the casts made. Many other individuals, together nearly 400 persons, have been carefully measured, and the zealous labors of other distinguished, particularly Indian, ethnologists, amongst whom we name Buist, Carus, Cunningham, Davis, Walter Elliot, Falconer, Hodgson, Hooker, Humboldt, Morton, Latham, Owen, Rawlinson, the Stracheys, Sykes, &c., will be found to have been carefully studied for the purposes of scientific generalization.

"We scarcely need add how important objects these facial casts will be for all those who take an interest in such researches, while the interesting nature of the objects themselves, as well as the careful and novel mode of their execution, will render them a most beautiful and important addition to public and private museums.

"This collection has met with great approbation; we mention as particularly important the well known personal interest of the late Baron v. Humboldt."

ADOLPHE SCHLAGINTWEIT'S DEATH IN TURKISTAN.—Our readers are already acquainted with the fact that one of the bold brothers, whose expedition to the Himalayas has attracted the attention of the whole scientific world, fell a victim to his energy. From the surviving brothers, Hermann and Robert, we have received a printed document in which are given all the official reports which have yet reached them in respect to the fate of Adolphe. The conclusions at which they have arrived are thus concisely stated.

"The information from India and Russia, collected from natives by European officers of the adjoining districts, concur but too accurately in establishing the fact, that Adolphe Schlagintweit was killed at Káshgar in Turkistán (Central Asia) in August, 1857, falling a victim to his scientific mission.

"He was recognized as a European after having passed the Karakorúm and Küenlüen, in disguise, where before us no European had ever travelled; he had taken a route more westerly than ours, and had succeeded in penetrating far into Central Asia.

"The reports which have reached us are so various, that they do not of course all agree, as to the immediate cause and particulars of his death; yet it is evident from all of them, that the political condition of these countries, and the circumstance of the deceased being recognized as an officer of the Indian Government, notwithstanding every precaution, essentially contributed to his tragic end. Even with the lively sympathy ever so energetically evinced by England, in the fate of scientific travelers, it will scarcely be possible to succeed in bringing the murderers of our brother to account.

"According to some reports he perished in consequence of having taken up the cause of some captive Bhot-Rajpúts, British subjects, interceding for them, that they might not be executed or sold as slaves. Other accounts state the immediate cause of his death was, his having been recognized as a European, and fallen by the hand of fanatic Mussálmáns.

"Notwithstanding our most zealous exertions for some months past, in endeavoring to obtain his manuscripts, drawings, etc., we have not yet been successful in learning anything definite about them: still, however, many very important geographical communications have been made to us by his followers, and we are not without hope that from the active sympathy which the Indian Government has always displayed in our scientific mission to India and High-Asia, nothing will remain untried that can tend to the rescue of his last papers."

LETTER FROM DR. LIVINGSTONE.—At the January meeting of the American Geographical and Statistical Society of New York, the following letter from Dr. Livingstone, the celebrated explorer in South Africa, was read by D. W. Fiske, Esq., Librarian of the Society:

Tette Zambesie, Feb. 22, 1859.

"*My Dear Sir*—Having been elected a member of your Society, I take the liberty to send you a short account of our attempt to open the interior of Africa, in the hope that, though it may not appear interesting to your members, it may, at least show my good will and desire to perform a corresponding member's duty.

We entered the delta of the Zambese in May, 1858, taking the most southerly branch we could find, but after ascending about seventy miles we found it impossible to enter the Zambese by that, as the points of junction were filled up with reeds and other aquatic plants. You may have a clearer idea of the region if you bear in mind the fact that the Zambese has in the course of ages formed a delta, which juts out into the ocean, and forms the most prominent part of the coast. The prevailing winds of these quarters beat, almost constantly, against the head of the promontory. These, aided by the oceanic currents, have helped to dam up the main stream, but the pent-up waters have escaped sideways. The main stream called Gualeo enters at the point of the promontory most exposed. To it we went after leaving the southern branch, but saw no possibility of entrance during three days, though her Majesty's ship Lynx has since found a channel in it, after a search of ten days. We then proceeded to examine the side branches and the very first we came to called Kongone, was all we required. There are other good ports, but all in the side branches. There are also communicating branches between these, and those within the influence of the tides are generally deep.

Having got into the main stream, we found that we had, in going to it and spending a month there, allowed the water to fall considerably. It was, also, so very much lower than usual that the Portuguese prophesied that we could not ascend ten miles. It was said, also, that war was raging, and no one would be allowed to go up, even if he could. Our ship drew nine feet seven inches, and she was under engagement to go to Ceylon. We, therefore, to avoid detention in the river, sent her off, and went up to the seat of war in a small steamer, drawing two feet six inches. We had no difficulty with the "rebels," as they were called—indeed, we got pilots from them, and continued ever after on the best of terms with the Portuguese. They were called "rebels," as they had all been runaway slaves, and bore the marks, in brands on their chests, of their former servitude. Slaveholders here must be civil, for it is so easy to run away, that if slaves go to the Landius, who are of the Zulu family, they never deliver them up. I have never heard of but one case to the contrary, and the owner—a great favorite of theirs—was obliged to give them his full value. This is a digression, but I may finish off by saying that the Portuguese governor attacked the rebels, and they retired before him, there being plenty of iron for all parties.

We continue carrying on luggage up the river till November, when it reaches its lowest point; and with care a flat-bottomed boat would do business even then. We know it now at its very worst, and, as it spreads out to from one to three miles in breadth, it is in many of the crossings not more than two and a half or three feet. Just now the water stands twelve feet above low water mark in November, and we are all quite sure that during at least eight months in each year a steamer of four or five feet could trade without embarrassment. The reason why so little has been known about the Zambese river, has been the branching in the stormy promontory by which it was hidden from navigators. And their easy chair geographers, dreaming over the geography of Ptolemy, actually put down the Zambese as flowing into the sea at Quilimane, which, in his days, it probably did, though not a drop of Zambese water, in ordinary circumstances, reaches that part. Had some branch of the Anglo-American family planted their footsteps on its banks, we are such a babbling newspaper set, the world would have known all about it long ago; and no one would have ventured to play with this river as has been done, making it lose itself and flow under the Kalobaro desert. You may form a better idea of its size if I tell you of one of the branches. We ascended the Shire lately, fully a hundred miles from the confluence, and found it with a two fathom channel all the way up. It varied from 80 to 150 yards in width, and contains no sand banks. It flows in a

beautiful fertile valley, about twenty miles high, and fringed with mountains of great beauty, well wooded to the top. Mora M. Vala we ascended, and found it 400 feet high. (This, by mistake, is placed on the wrong side of the Shire, in my map.) It was well cultivated on the top, and had several fine little fountains, the waters of which were slightly chalybeate; they have a hot sulphurous fountain at the base, (temperature 174° Fahrenheit). The people had many sweet potatoes, holcus sorghum, and other grains, and pine apples, lemon and orange trees. They were very hospitable, and independent. The vegetation is very different from the plains, and so is the climate; yet with all these disadvantages, no use has been made of it as a sanitorium by the Portuguese, and as far as we can ascertain, this river has never been explored by Europeans before. One part of the luxuriant valley of the Shire is marshy, and abounded in lagoons, in which grow great quantities of the lotus plant. The people were busy collecting the tubers, which, when boiled or roasted, resemble chestnuts. They are thus Lotophagi, such as are mentioned by Herodotus. Another part of the valley abounds in elephants. My companions estimated the numbers we saw at eight hundred. Herd upon herd appeared as far as the eye could reach; and noble animals they were. We sometimes chased them in our little steamer, for the shore branches off occasionally and forms islands. The upper part of the valley is well peopled, and many of the hills are cultivated high up. But never having seen Europeans before, they looked on us with great suspicion. They watched us constantly, well armed with bows and poisoned arrows, ready to repel any attack, but no incivility was offered when we landed, nor were our wooding party molested. We obtained what may be considered reliable information that the Shire actually does flow out of Lake Nyanga. We were brought up by a cataract, but five days beyond this point the water is smooth again, and Arabs come down in canoes from Nyanga thither. Seeing the suspicions we had aroused, we deemed it unsafe to leave the vessel and go overland. But no collision took place. The greatest coward fires first, so, thinking we had as much pluck as they, we did not lift a gun, though we were ready to fire, or rather shoot. We did nothing to make us ashamed to return, and mean to do so next month; and if we have their confidence we may go farther. They had abundance of provisions, and sold them at a cheap rate. Also cotton of two kinds—one indigenous, short in the staple but very strong, and woolly to the feeling; the other very fine, and long in the staple. We brought a number of specimens of their spindles and yarn, and it was quite equal to American uplands; did not offer them any American seed. The cotton plant is met with everywhere, and though burned down annually springs up again as fresh and strong as

ever. They grow sugar cane too, bananas, &c. The men are said by the Portuguese to be very intelligent, but very mild. The women wear the lip ornament, round one of which I put my pen. The slit is made in the upper lip, at first, by a ring in childhood. The ends are gradually pressed closer together, and cause absorption till a hole is made. This is enlarged by bits of reed, until in a lady of fashion a ring, either hollow or cup shaped, is inserted, and the edge of the lip protrudes beyond the perpendicular of the nose at least an inch. I am thus particular in case our own ladies, who show a noble perseverance when fashion dictates, may wish to adopt lip ornaments.

Above this we have a rapid, called Kebra, or rather Kaora-basa. When the water is low it shows a deep grove, with perpendicular sides. When steaming up this the man at the lead kept calling "no bottom" at ten fathoms, and the top of the walls of the grove towered from 50 to 80 feet above our deck. It is from 60 to 80 yards wide, but at this season is comparatively smooth. There were some cataracts in it which high water obliterates. This steamer is too weak to ascend. She being only ten horse power, and her plates $1\frac{1}{16}$th of an inch thick, we dare not try her in the rapids. We shall work down here some time yet. I long to lead back my faithful Mackalolo, who are still at Telle, though thirty of them died of the small pox, and six were killed by a neighboring chief.

I shall refer to one point more before concluding. We were warned by the fate of the Niger expedition not to delay among the mangrove swamps of the Delta—the very hot beds of the fever. We accordingly made all haste to get away, and we took daily a quantity of quinine. The period of the year I selected, though not the most favorable for navigation, was the most so for health; and, thank God, our precautions were successful.

The Kroomen from Sierra Leone have had more of it than we, until a short time ago, when, it being the most unhealthy season of the year, and even to the natives, three of us have had touches of the complaint, but are all now quite well. I have never had a day's illness since my return. We find, too, that so far from Europeans being unable to work in a hot climate, it is the want of work that kills them. The Portuguese all know that so long as they are moving about they enjoy good health, but let them settle down and smoke all day, and drink brandy, then—not a word about brandy in the fever that follows—the blame is all put on the climate. I am, &c. DAVID LIVINGSTONE.

KRAPF'S RESIDENCE AND TRAVELS IN EASTERN AFRICA.—Messrs. Trübner & Co. of London announce as nearly ready for publication a work which is likely to rival in interest the recent volume of Dr. Livingstone. We refer to the narrative of a Missionary Residence in Abyssinia by Dr. J. L. Krapf—one of

the agents of the Church Missionary Society of London. It will be recalled by our readers that it was by him and his comrade Rebmann that intelligence was first given to the civilized world of the possible existence of snow-covered mountains near the equator—the famous Kilimandjaro. The observations of these missionaries have given rise to many warm discussions, and the correctness of their opinion has been earnestly disputed. In the forthcoming volume we may anticipate that this controverted point will be examined with thoroughness and detail. Aside from this discussion, the work of Dr. Krapf will abound in interesting comments upon his missionary life. His land journeys, which were mostly upon foot, extended 9000 miles. We quote the following from the prospectus of this work.

"Two things may be said of Dr. Krapf which can be affirmed of no other modern African traveler. He has traversed Abyssinia from north to south and from east to west; and further, he has explored the whole coast of Eastern Africa, from Suez to the 10th degree of south latitude, and inspected every place of importance to be found on it. Such journeys and voyages would alone bestow a high value on a volume like the present one, which communicates their most important results. But more than this, the large and interesting country which stretches from the Equator to the 5th degree of south latitude was, from the eastern coast inwards, all but a *terra incognita*, until it was traversed, on foot, by Dr. Krapf, and by his colleague and fellow-worker, the Missionary Rebmann, whose experiences are also included in this work. From the Mission-station at Rabbia Mpia, on the coast, these brave and fearless men prosecuted journeys for at least three hundred miles into the interior, exposed to every possible peril and privation. These journeys were repeated by different routes—the dangers incurred on one seeming only to stimulate to self-exposure to greater dangers on another. Rebmann's three journeys to Dschagga, Krapf's two journeys to Usambara, and two more to Ukambani, in the course of which they explored regions and visited—Bible in hand and Gospel on lip—populations never before seen by European, have rarely been exceeded in interest—religious, adventurous, and geographical. The story of Dr. Krapf's abandonment and wanderings in the wilderness, during his second journey to Ukambani, carries the reader back to the old days of adventurous travel. Scarcely in the whole annals of modern missionary effort has there been anything equal to the spectacle displayed in this section of the volume, of two individuals, each isolated, pursuing again and again, on foot, without external encouragement of any kind, and in the face of every possible obstacle, journeys among ignorant and savage heathen, far away from help, or the hope of help, and confiding solely in the guidance and support of Providence. The

splendid geographical and ethnological results which were among the rewards of these daring pilgrimages will be found fully chronicled for the first time in the present volume."

SPEKE'S EXPLORATIONS IN EASTERN AFRICA.—At a recent meeting of the Royal Geographical Society of London, Capt. Burton and Capt. Speke both gave a narrative of their explorations in Eastern Africa, which are of particular importance, as our readers are well aware, in connection with the long disputed problem of the sources of the Nile. So much interest has been manifested everywhere in this expedition that we regret that our limits will not permit us to reprint entire the discussion to which these two papers gave rise in that learned association. Sir R. I. Murchison, Col. Sykes, Mr. Macqueen, Mr. Galton, and other well known gentlemen presented their views upon this important topic, a report of which will be found in the Society's Proceedings, vol. iii, No. 6. From the same source we extract the following statement of the remarks of Capt. Speke.

"The region traversed by Captain Burton and myself is divisible into five bands. They all run parallel to the coast, and each of them is characterised by special geographical features. The first is low land between the coast range and the sea. Its breadth is about 120 miles, and its average slope not more than 2 feet per mile. Forests of gigantic trees, and tall grasses, cover its surface. The second band is the coast range of mountains. These are hills in lines and in masses, intersected by valleys, through which the rivers of the east coast find their way. This range is easily crossed, and nowhere exceeded 6000 feet, adjacent to the line of road taken by our travelers. It is capable of cultivation, though neglected, because the slaving forays to which it is subjected drive away the inhabitants. The third band reaches to Unyanyembe. It is a dry plateau, with a slight inclination toward the interior, and ranging in height between 3000 and 4400 feet. Tributary streams, running southwards to the Ruaha, intersect it. The fourth zone is a continuation of the above, but it is better watered, and is studded with granite hills. Here is the water-parting between the streams that run eastward to the Indian Ocean, and westward to the Tanganyika Lake. The Nyanza Lake is situated in this band. The fifth band is a remarkable slope, that inclines to the shores of the Tanganyika. It sinks no less than 1800 feet in 45 miles; it is exceedingly fertile, but harrassed by marauders of the Watuta tribe.

On arriving at Ujija, the party found that the only boats to be had were wretched canoes; while the troubled state of the country rendered it unsafe to explore the lake unaccompanied by a large escort. There was, however, a small sailing craft belonging to an Arab, on the other side of the lake, which would be large enough to contain the entire party; and Captain Speke started

to hire her, with seventeen savages, as a crew, and four of his own men. He first coasted to Kabogo, a bold promontory usually selected as the starting point, when the lake has to be crossed, and reached it in five days. He describes the shore as wild and beautiful, affording many convenient harbors, and requiring but a little art to make it quite a fairy abode. There were no inhabitants, but an abundance of game,—hippopotami, buffaloes, elephants, antelopes, and crocodiles. The passage across the lake, a distance of 26 miles, was made rapidly and safely, and Captain Speke was cordially welcomed by the Sultan of the country on the opposite side. The owner of the sailing boat was there also, and was ready to afford every assistance; but he himself was on the point of starting on an ivory expedition 100 miles into the interior, and the crew of his sailing boat were, at the same time, his armed escort: he could not therefore spare them. What made the disappointment doubly vexatious, was that this Arab desired Captain Speke's companionship in his intended journey, and he promised the boat on his return. Had Captain Speke been unfettered by time, this would have been an excellent opportunity of farther travel. As it was, he was obliged to go back to Ujiji without the sailing boat, and proceeded with Captain Burton to a more extended exploration of the Tanganyika Lake, which lasted a whole month. The mapping of its southern portion depends on information given by this Arab.

On returning to Unyanyembe, Captain Burton's continued illness again made it necessary for Captain Speke to proceed alone to the northward to explore the Lake Nyanza. He went with thirty-three men, through a line of populous country, less visited by strangers than that which he had hitherto traveled on. There were numerous petty sovereigns who were hospitable enough but very troublesome. The view of Lake Nyanza, with its numerous islands, reminded Captain Speke of the Greek archipelago. The islands were precisely like the tops of the same hills that studded the plains he had just traveled over. In fact, the lake had the features of a flooded country rather than those of a sheet of permanent water, with well marked banks. Its water is sweet and good: those who live near it drink no other.

Captain Speke's explorations did not extend beyond its southern shores. The more northern part of his map is based on native information, especially on that of a very intelligent Arab, whom he has previously met with in Unyanyembe, and whose data, so far as the shores of the lake, were found by Captain Speke to be remarkably correct. This Arab had traveled far along its western shores. In thirty-five long marches he reached the Kitangura river, and in twenty more marches, Kibuga, the capital of a native despot. Between these two places he crossed

about 180 rivers, of which the Kitangura and the Katanga were the largest. The former is crossed in large canoes; the latter, though much larger and broader, is crossed during the dry season by walking over lily leaves; but in the wet season it spreads out to an enormous size, and is quite unmanageable. The rainy season is very severe in these parts. No merchants have gone farther than Kibuga; but, at that place, they hear reports of a large and distant river, the Kivira, upon the banks of which the Bari people live. This river is believed by Captain Speke to be the White Nile."

Sir R. I. Murchison in reviewing the labors of the two explorers, remarked that "they have, by means of astronomical observations, fixed the position, the longitude and latitude of these two great lakes, and have shown you that whilst one is like other lakes, of which we had previously heard, situated on a great plateau, the other is situated at such an elevation that, as Captain Speke has explained to you, it may very possibly be found to feed the chief sources of the Nile. I will not now argue that difficult question, because I am quite sure there is one gentleman here, if not others, who may dispute that inference. I will, therefore, first call attention generally to the great importance of these discoveries. My friends here have not only traversed the district and furnished us with a good picture of the manners and customs of the inhabitants, but have also brought home rock specimens which enlighten us as to the fundamental features of this country; and to these rocks I will for a moment advert. Captain Burton placed before me this morning certain specimens which show me that at an elevation of upwards of 3000 feet above the sea and towards the interior there are fossilized land shells, showing that from very ancient periods the lands have maintained their present configuration. These deposits, whether purely terrestrial or lacustrine, have been consolidated into stone, and show that the existing internal condition of Africa is that of ages long gone by, as I took the liberty of pointing out to the society some years ago, when treating of Livingstone's first explorations. Another striking feature in connection with this great zone of country is this. You will observe that our friends spoke of remarkable herds of oxen on the banks of the lake Tanganyika, and tribes of people between that vast lake and the coast range, who are a thriving, peaceful, agricultural population, whilst the adjacent districts in the north and south are frequently disturbed by wars for slave-hunting purposes. This is a great fact as indicating a broad line of route by which we may hope hereafter to establish intercourse with the interior country. There is another important fact, though I do not think Captain Speke alluded to it, namely, the absence of that great scourge of parts of southern Africa, the *Tsetse* fly.

With regard to the physical geography of the country, it is remarkable that all the adjacent rivers fall into the great Tanganyika lake, which was formerly supposed, on the contrary, to afford the sources of the Zambesi river. All theory, therefore, on this subject is now set at rest. Lastly, we come to the subject which is likely, as I said, to give rise to much discussion, and that is the theory upon which I think my friend Captain Speke may rest his claim to our most decided approbation. On my own part I am disposed to think that he has indicated the true southernmost source of the Nile. Now, in saying this I do not mean to deny that the great mountains flanking the lake on the *east*, of which a point or two only is marked on the map before us, do not afford the streams which flow into this great lake. That must probably be the case on the east, just as Captain Speke ascertained from the Arabs that the so called "Mountains of the Moon" feed the same lake from the west by other streams. You must here recollect that the same Arab sheik who gave him the information which turned out to be correct concerning the existence of the lake Tanganyika also told him of the existence of the Nyanza, which lake was found to be exactly in the position indicated. As Captain Speke has determined that this great lake Nyanza is nearly 4000 feet above the sea, it may well, indeed, be the main source of the White Nile. Everything (as far as theory goes) being in its favor, this view is farther supported when we reflect on the fact that the tropical rains cause these upland lakes and rivers to swell and burst their banks at a period which tallies very well with the rise of the Nile at Cairo. These, then, are grounds which I think must go to strengthen the belief of Captain Speke, and I may, therefore, repeat what I stated at the anniversary, that highly worthy as Captain Burton was to receive a gold medal, not only on account of this great expedition which he led, but also for his former gallant and distinguished expeditions, Captain Speke, who now sits at your Lordship's left hand, is also entitled to a gold medal of the Royal Geographical Society."

H. Schlagintweit on the Salt Lakes of the Himalayas.—At a recent meeting of the Royal Geographical Society of London, Mr. H. Schlagintweit exhibited some chromo-lithographic sketches of the Himalayan Mts., and in commenting upon the remarkable erosion which takes place upon that range, he spoke as follows of the salt-lakes which form a peculiarity of that region:

"Another consequence of the erosion is the gradual drainage of fresh water lakes, or their conversion into salt water lakes. It is very characteristic for the Himalayas, and in this respect they differ essentially from most other mountain systems in the world, that hardly any fresh-water lakes now occur. The only

few lakes of any considerable extent which have been made known by Captain Strachey, Captain Speke, and Major Cunningham, as well as those we visited besides, are all salt water. But the explanation we think we must give of this phenomenon is different from the explanation formerly given. Some have thought that a raising of the country might have caused a general drainage. We think that supposition rather improbable, from the recent strata round these salt lakes being all horizontal, and the outlets of these salt lakes being in a different direction in reference to the horizon. If any raising of the country had effected the drainage of the salt lakes, the effect would have been a perfectly different one, according to the position the outlet of these lakes had in reference to the points of the horizon, a modification which is nowhere met with.

"The Tso mo Ri ri and the Tso mo Gnalari, the two great salt lakes of Rupchu and Pankong, of which drawings are presented, happen to be a good example of two large lakes, being about equally salt, with differently directed former outlets, and with quite horizontal banks of detritus and of watermarks along their circumferences. The gradual progress of the erosion of the valleys seems to us to be also the chief cause of the gradual transformation of freshwater lakes into saltwater lakes in Tibet.

"By this progressive excavation thousands of square miles, still marked as former lakes by the form of the surface, have been emptied, and the consequence is that the local evaporation could no more keep the equilibrium with the precipitation; in consequence the lakes, of which parts remained undrained on account of their greater depth, now gradually became more and more salt."

Journal of the Royal Geographical Society of London.—We have just received the twenty-eighth volume of the Journal of the Royal Geographical Society of London. Like the previous parts of this series it is full of important contributions to our knowledge of the physical geography of every country where British enterprize is manifested. We have heretofore quoted from the anniversary address of the President, Sir R. I. Murchison. To many of the other articles we shall have occasion to allude. The following is a statement of the contents of the volume:

Articles—1. Journal of the North Australian Exploring Expedition; under the command of Augustus C. Gregory, Esq. (Gold Medallist, R.G.S.); with Report by Mr. Elsey on the health of the party.—2. Notes on the Physical Geography of Northwest Australia; by Mr. James S. Wilson, Geologist to the North Australian Expedition.—3. Journey from Colesberg to Steinkopf in 1854–5; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—4. Journey from Little Namaqualand eastward, along the Orange River, the Northern Frontier of the Colony, &c.,

&c., in August 1856; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—5. A Coasting Voyage from Mombasa to the Pangani River: Visit to Sultan Kimwere: and Progress of the Expedition into the Interior; by Captains Richard F. Burton, commanding the East African Expedition, and J. H. Speke, F.R.G.S.—6. Explorations in the Desert East of the Haurán, and in the Ancient Land of Bashan; by Cyril C. Graham, Esq., F.R.G.S., &c.—7. Contributions to the Knowledge of New Guinea; by Dr. Salomon Müller,—8. On the supposed discovery, by Dr. E. K. Kane, U.S.N., of the North Coast of Greenland, and of an Open Polar Sea, &c., as described in the 'Arctic Explorations in the years 1853, 1354, 1855;' by Dr. Henry Rink, M.D., Inspector in Greenland for the Danish Government.—9. The Yang-tse-Keang, and the Hwang-Ho or Yellow River; by William Lockhart, Esq., F.G.R.S.—10. Extracts from a Journal kept during a Reconnaissance Survey of the Southern Districts of the Provinces of Otago, New Zealand; by J. Turnbull Thomson, F.R.G.S., Chief Surveyor.—11. Observations relative to the Geographical Position of the West Coast of South America; by Carlos Moesta, Director of the National Observatory, Santiago de Chile, May 29, 1856.—12. Excursion made from Quito to the River Napo, January to May, 1857; by Dr. William Jameson.—13. Description of the State of San Salvador, Central America; communicated by John Power, Esq., F.R.G.S., of Panama.—14. On the Latitude and Longitude of some of the principal places in the Republic of Guatemala; by A. van de Gehuchte.—15. On the Fine Regions of the Trade Winds; by Thomas Hopkins, M.B.M.S., Vice-President of the Manchester Literary and Philosophical Society.—16. Remarks upon the Amount of Light experienced in high Northern Latitudes during the absence of the Sun; by Captain Sherard Osborn, R.N., C.B., F.R.G.S., Officier Légion d'Honneur, &c.—17. Notes on the River Amúr and the adjacent Districts; by MM. Peschurof, Permikin, Shenurin, Vasilief, Radde, Usoltzof, Pargachefshi, &c.

Illustrations.—1 and 2. Map to illustrate the Route of the North Australian Expedition, and Mr. Wilsou's Paper on the Physical Geography of N.W. Australia.—3 and 4. Map to illustrate Mr. Moffat's Journey from Colesberg to Steinkopf; and from Little Namaqualand Eastward, along the Orange River.—5. Map to illustrate the Progress of the East Africa Expedition.—6. Map to illustrate Mr. Cyril Graham's Explorations East of the Haurán, &c.—7. Map to illustrate Dr. Rink's Paper on Dr. Kane's Arctic Explorations.—8. Map to illustrate Mr. Thomson's Survey of Otago.—9. Map to illustrate Capt. Sherard Osborn's Paper on Light in the Arctic Regions.—10. Map to illustrate Notes on the River Amúr.

Yale College Library, March, 1860.

ART. XXIII.—*On the Species of Calceola found in Tennessee: Calceola Americana;* by Prof. J. M. SAFFORD.

FOR many years it has been known that a species of *Calceola* occurs in the marly and glade-forming limestones of Western Tennessee. This species has been considered to be identical with the European *C. sandalina* of Lamarck, an error (for such I hold it to be) which has contributed much to the confusion that has existed with reference to the age of the limestones mentioned. Individuals of the species are frequently found upon marly glades of Decatur, Perry, Wayne, and Hardin counties. The identity of the species with *C. sandalina* (and a few other determinations of the same kind) once taken for granted, it was an easy matter to designate the rocks of these glades "Devonian."

Since my attention has been called particularly to this species, I have regarded it as distinct, and now propose for it the name *Calceola Americana.*

In the first place, its different geological position would, at least, indicate a distinct species. It is without doubt an Upper Silurian fossil, and moreover belongs exclusively, so far as my observations have extended, to the Niagara Period.* The grey marly limestones of *the glades*, although much alike lithologically, are generally easily separable, by their fossils, into two beds, the lower one representing, in part, the Niagara Period, and the other the Lower Helderberg. It is to the former of these that our *Calceola* belongs. Among its associates are *Orthis elegantula, Platyostoma Niagarensis, Caryocrinus ornatus, Eucalyptocrinus decorus*, &c. *Halysites escharoides* and *Cladopora reticulata* have been observed in a local coralline limestone resting upon the bed containing the *Calceola.*

In the second place, the characters which separate it from *C. sandalina* are well marked. In general form, it is much like the European species, but differs in the following particulars:†

1. In *C. sandalina* the central cardinal process or tooth of the large valve is divided longitudinally by a shallow linear groove, making the tooth apparently double; in all my large valves of *C. Americana* this tooth is not grooved, but, on the other hand, is rounded and smooth along its summit; it is moreover longer and larger than in the European species.

* The *Niagara Period*, as here used, is equivalent to the New York rocks from the Oneida Conglomerate to the Niagara Group inclusive.

† In making these comparisons I have before me forty specimens of the American species and seven of the European. Of the first, one is an excellent specimen with both valves united, two are good specimens of the small valve, and the remainder are large valves. The small valves are seldom found. I have seen altogether four of them. Of my European specimens, one is entire with both valves, another is an excellent small valve, and the rest are large valves all in good condition.

2. In my specimens of *C. Americana* the rows of punctures, so conspicuous on the internal surfaces of *C. sandalina*, are not seen.

3. Within the largest valve of our species, in the older individuals, there is adjoining the hinge line, and on each side of the cardinal process, (but separated from the latter by a deep groove,) a prominent callosity. In very old specimens, these callosities nearly fill up the back portion of the cavity of the shell, and, at the same time, nearly obliterate the striæ or ridges which run forward from the hinge line. Most of the inner surface of the large valve has an irregular wavy appearance, indicating a vesicular structure, which, in fact, the mass of the valve has.

4. The small valve (the dorsal), so far as I have seen, has externally no proper cardinal area; its *apex* is not immediately over the hinge line, but is removed about one-fourth of the length of the valve towards the front, the cardinal edge being bevelled off from the apex to the hinge line. The lines of growth are prominent along this bevelled edge; so they are too on the cardinal area of the large valve.

5. The external surface of *C. Americana* is obscurely marked longitudinally in front by striæ, which, so far as they have been seen, are coarser and less numerous than in *C. sandalina*.

There are other points of difference which appear to be constant, but those given are sufficient to characterize the species. Upon a future occasion the fossil will be illustrated by the proper figures.

Lebanon, Tenn., Feb. 1, 1860.

Art. XXIV.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—3d Article.

In the two preceding numbers of this Journal* we have given observations of the Aurora of Aug. 28th to Sept. 4th, from numerous places in North America. We now continue our record of the phenomena, and intend in a subsequent number to present a summary of the observations made in other parts of the world. We are indebted to Mr. Benj. V. Marsh, of Philadelphia, for a considerable number of the following notices.

1. *Observations at Montreal* (lat. 45° 31′), *by* Dr. Archibald Hall.

August 28th about $8^h\ 20^m$ P. M. the sky was about seven tenths obscured by massive cumuli, when in the interval between them I observed streamers of a ruddy tint passing from the south towards the zenith. The wind was N.N.W. and blowing

* Vols. xxviii, p. 385; xxix, p. 92.

rather stiffly. About 10 P. M. the streamers seemed to converge towards the zenith in all directions, and to possess a deep ruddy tint. There was a large cumulus cloud in the W.S.W. and from a clear space beneath it a streamer shot upwards and distinctly traversed the cloud, illuminating it vividly. The same phenomenon was witnessed by another observer at the other end of the city.

At $2^h\ 10^m$ A. M. Sept. 2d, a brilliant aurora was seen in the vacant space between masses of huge cumuli and lasted until $3^h\ 30^m$ A. M. The sky was at first of a bright coppery red tint, and the light emitted so great that it was possible to read moderately large print by it. This space became interspersed with streamers of a rich roseate hue stretching to the zenith. The manifestation was chiefly observed in the W.S.W.

Sept. 2, at $9^h\ 40^m$ P. M. we had another auroral display. The streamers were mostly white, springing from three well-defined arches, stretching between the N.E. and N.W. They flickered magnificently about $10^h\ 20^m$ P. M. in the zenith, where they formed a huge corona having a tent-like appearance. These displays have been the finest seen here for many years, and it is to be regretted that on the two first occasions, clouds should so far have concealed them from our view.

2. *Observations at Montreal* (lat. 45° 31′), *by* Prof. CHARLES SMALLWOOD, LL.D.

Aug. 28th at 9 P. M. we had a splendid aurora extending over nearly the whole horizon with the exception of a small space in the south and S.W., varying in color from a pale yellow to deep orange and violet or crimson, and nearly as light as when the moon is at its full. The aurora was first noticed between $8^h\ 30^m$ and 9^h P. M., and this appearance lasted, with modifications, till nearly sunrise.

On the following night, Aug. 29th, there was also a fine display, but not to be compared in brilliancy to that of the previous evening. The sky was on this occasion cloudless, and a few streamers were occasionally seen tinted with a pale violet color.

The most remarkable incident was the unusual amount of atmospheric electricity present. At 9 P. M., Aug. 28th, the electrometers indicated a maximum of 250 degrees in terms of Volta's electrometer No. 1, of a positive character (but almost constantly varying in intensity); an amount equalled only during the thunder storms of summer, and the heavy snow storms of winter. The amount during the following day and night indicated a maximum of ten degrees, which is however somewhat above the usual average.

The appearances would lead to the opinion that the clouds might have been the medium of conducting the atmospheric

electricity to the earth, for the indications of the electrometers were such as are observed during the passage of clouds charged with electricity, and this phenomenon seems to have extended to the wires of the electro-magnetic telegraph.

The following day and night indicated a small increase on the usual amount of electricity, which may be owing to the continued presence of the aurora, or in some measure to the decrease in temperature.

Similar indications of the electrical state of the atmosphere during the aurora were never observed here, although its effect on the magnetic telegraph has been before witnessed.

3. *Observations at St. Paschal* (lat. 47° 40′ N., long. 67° 40′ W.), *communicated by* Prof. C. Smallwood.

It was about 10 P. M. Aug. 28th, that the aurora was first noticed here. It was a magnificent display which threw out streamers from the zenith all around the horizon, and the light was nearly that of the day. I believe it was visible at Lake St. John on the Saguenay, lat. 48° 8′, long. 71° 9′.

4. *Observations at Halifax* (lat. 44° 39′), *by* Lieut. N. Home, *of the Royal Engineers.*

Aug. 28th at 5 P. M., I remarked a long narrow belt of cloud from E. to W. having a peculiar orange-white appearance.

At 8 P. M. I observed this cloud (which in the interim appeared to be stationary) suddenly to become luminous, particularly at its eastern extremity. This cloud was about 10° wide, and appeared to extend from horizon to horizon; no other clouds were visible.

Soon after 8 P. M. two arcs of light N. and S. appeared, that to the south being the brightest. Under both these arcs the heavens were dark; but observers were uncertain as to whether the darkness was cloud or not. No stars were seen below the arcs, although quite visible above them.

At 9¼ P. M. the appearance was as if these two arcs were a small circle of the sphere, dipping to the south at an angle (measured by sextant) of 15° to the horizon, and 12° above it. The corona being formed at a point (by sextant) 10° south of zenith. There was only one band or arc of light, and that was continuous around the whole heavens. There were two remarkable patches, one due west, at an elevation of about 36°, having a red color; and the other east by north, at an elevation of 25°, having an orange color. These points were brightest during the whole display.

Two distinct sets of streamers appeared to be formed; one set from the arc of light, the other from the corona, which seemed to be constant or nearly so; as during the five hours I watched

the aurora, there appeared to be always light in or near the zenith, and always in the arc. The streamers were the variables, and appeared to work from W. by N. to south. I think they worked along from E. to W., but another observer said from W. to E. To the south they were so vivid and rapid it was not easy to tell.

A volume of light, as if a quantity of burning spirit was poured over the heavens, appeared to stream across from north to south quite independent of the streamers. The corona sent down rays, but it seemed to be only half way; the streamers from the arc meeting them and toothing in, appearing to alternate, short and long ones.

5. *Observations at Grafton, Canada West* (lat. 44° 3′ N., long. 78° 5′), *by* JAMES HUBBERT.

Aug. 28th at $8^h\ 30^m$ P. M. my attention was attracted by the peculiar appearance of the southwestern sky. Streamers and flashes of light of a pale yellow and red color were rising, sailing towards a point 8° south of the zenith, and meeting others from the N.W. and north. By $8^h\ 53^m$ the whole northern and eastern sky was a blaze of lurid light, which seemed most dense in a band seven degrees wide, extending from N.W. to S.E., along which there was a constant succession of streamers and nebulous patches, exhibiting every shade of white, yellow and red. Columns were now darting up from all parts of the horizon. The aurora hung along the south, in a line at a maximum height of 17°. This from $8^h\ 50^m$ to 9^h was very perfect; while a similar arch but much less regular was formed in the north, reaching to the east. The latter had an altitude of 27°, and like the other seemed to rest on a dark bank. The first corona that I observed was formed at 9^h, at an altitude of 70°. It was imperfect and vanished almost instantly; but was soon replaced by another in nearly the same spot. This in turn gave place to another still more complete. From $9^h\ 15^m$ to $10^h\ 15^m$ the drapery was gorgeous in the highest degree. A diffused light made surrounding objects very distinct. Cocks crew, and the animal world seemed to think that day was dawning.

I noted constant changes which were little more than a repetition of the above till 3^h in the morning. The corona was distinct from $12^h\ 37^m$ to $1^h\ 5^m$. The color was white, merging into every shade of yellow, crimson, scarlet, purple, and sometimes tinged with green. I listened with great earnestness, and once or twice thought I heard a rustling noise, but I think it must have been the wind. When the wind was hushed, as it was at intervals in the latter part of the night, not a sound could be heard. Just at 10^h the aurora, after nearly disappearing, became intensely brilliant, equalling the light of the moon at the

last quarter. The aurora continued till daylight, when it gradually faded away.

The evening of Aug. 29th was clear; and at $8^h\ 45^m$ the aurora was again visible, but very much less extended and brilliant than on the preceding night. There were thin, misty clouds of a nebulous appearance, with occasional streamers of a pale white light, sometimes merging into red.

On the night of Aug. 30th I observed no unusual appearance.

Aug. 31st the sky was covered with a dense mass of clouds; but the existence of the aurora was evident from the clearness of the night. After midnight the clouds disappeared, and the display was magnificent. All the characteristics of the night of the 28th were repeated; but the arch was rather lower along the southern horizon. A fiery bank was formed in the south, from which rays were constantly darting upward, and the whole sky was a gorgeous canopy of crimson and gold. This was most vivid from $1^h\ 15^m$ to $1^h\ 45^m$, but was continued till almost daylight.

Sept. 1st was cloudy, and I saw no indications of the aurora.

Sept. 2d there were dense clouds, yet the aurora might be occasionally seen. It was confined to the N. and N.E., and was particularly bright from $9^h\ 51^m$ to 11^h.

Sept. 3d was clear. At $8^h\ 50^m$ the aurora appeared in the N.N.E. and W. The light was yellow and white, with traces of crimson and green. At 10^h an imperfect corona was formed, but almost instantly disappeared. Others followed, but none of them were complete.

Sept. 4. The same phenomena were observed, but much diminished in brilliancy.

Sept. 5. No trace of the aurora was visible.

6. *Observations at Rochester, N.Y.* (lat. 43° 8′), *by* Prof. C. DEWEY.

The aurora of Aug. 28th was exceedingly splendid both before and after midnight, with the corona a little south of the zenith; and exhibited many colors, with red or crimson predominant.

Sept. 1st. The aurora began late in the evening, and exhibited the usual appearances.

Sept. 2d at 1 A. M. it was cloudy, but very bright and red in the N.E.; the light increased rapidly and extended. At 2 A. M. there was a magnificent glow of red over the southeast, south and southwest; yellowish green, green and crimson, forming a gorgeous display quite down to the south horizon as seen from the housetops. A splendid corona was formed just south and east of the zenith, with splendid coruscations from towards the horizon up to the zenith. The streamers shot upward towards where the corona was formed, but none went to it.

Sept. 3d. The aurora was considerable at 9 P. M., and over the north, streamers were shooting upwards. At 10 P. M. there was a bright red space in the N.W. or W.N.W. with white and greenish-white bands on each side. The flashing of light upwards soon began, and the streams or clouds of aurora were splendid. At 10½ P. M. the corona began a little S.E. of the zenith, and was very splendid, towards which the streaming upwards was on all sides but less from the south. At 11 P. M. it nearly disappeared. This aurora was equal to that of November, 1837.

7. *Observations at Newburyport, Mass.* (lat. 42° 48′), *by* Dr. Henry C. Perkins.

The aurora of Aug. 28th was the most splendid ever witnessed at Newburyport by the present generation. About 7½ P. M. the eastern sky seemed to outvie the western, but with reversed colors, the pink of the morning taking the place of the golden hues of the setting sun. In a few moments these hues were repeated in the northeast and the west, and the yellowish-white luminous arch had passed the zenith and was fast covering the southern sky, and at 7¾ P. M. had enveloped Antares. At $7^h\ 52^m$ the star Tau Scorpii was at the southwestern edge of the luminous fringe. At 9 P. M. Lambda Scorpii marked its southern border. At 9¼ P. M. the northern border of a luminous arch passing from the east to the west, was marked by Nu in the right foot of the Swan, while the whole southern and southwestern heavens were glowing with streamers rushing to the pole of the Dipping-needle, the whole northern heavens being entirely destitute of the auroral light. At this juncture, in an instant as it were, the merry dancers sprang up from the northern heavens, and at 10 P. M. the whole celestial vault was glowing with streamers, crimson, yellow, and white, gathered into waving brilliant folds, a little to the south and east of the zenith, affording a canopy of the richest tints and most magnificent texture. The light was examined by the polariscope, and found not polarized. The stars were so lost amid the effulgence as to render it somewhat difficult to make out the constellations. Print might be read by the aid of a small lens, and the time ascertained from the watch by the simple light of the aurora.

During the evening of Sept. 1st the aurora was quite bright, and about a quarter to one (Sept. 2) it spread very rapidly, and soon enveloped the whole heavens. At about one the spectacle was magnificent, a perfect dome of alternate red and green streamers being formed, and the light being so great that ordinary print could be read as easily as in the day-time. It continued till morning.

8. *Observations at Lunenburg, Mass.* (lat. 42° 35′), *by* Prof. WILLIAM B. ROGERS.

The aurora of Aug. 28th has rarely been equalled in this latitude, and the meteor was repeated with more or less splendor for the eight following nights. The displays of Sept. 1st and 2d were scarcely inferior in beauty to that of the 28th, while that of Sept. 2d, in some of its features, was the most interesting of them all.

On the evening of Aug. 28th, throughout most of the northern half of the sky, the stars were dimmed by what seemed to be a luminous haze, which in some places quite eclipsed their light, and which itself glowed changefully with a golden and crimson coloring. In the earlier stage, the obscure space on the northern horizon had not assumed the usual arched form, and was sufficiently translucent to show a few flaky clouds, floating within its confines. At $8^h\ 20^m$ this dark space had become more opaque, and had moulded itself into a symmetrical arch, bounded by a broad luminous band.

At $9^h\ 30^m$ the display attained its highest magnificence. The dome of the heavens was hung around with white and golden and rose-tinted streamers converging from all quarters towards the magnetic pole. Over the glowing stripes of this marvellous pavilion there came broad flushes of the richest crimson light, until it suffused all the upper part of the sky, and the whole southern quarter except a narrow space next the horizon.

At $10^h\ 30^m$ nothing remained of this wonderful spectacle but a faint auroral arch low down in the north, accompanied by a few dim streamers.

The aurora recurred in great splendor between 1 and 2 A. M., Aug. 29th, when the crimson color was particularly remarkable. At $3^h\ 30^m$ A. M. there was a fine auroral arch in the north, with a long array of streamers rising from it.

Sept. 2d, a clear sunset was followed by a peculiar greenish and purplish light extending round the horizon, even beyond the north. Over the northeast quarter, the air to the height of 30° had a dark opacity, which had the effect of arresting the light coming from beyond.

At $7^h\ 30^m$ P. M. an irregular obscure space began to form along the northern horizon. At $7^h\ 50^m$ a faint arch of white light made its appearance, resting on the horizon a little north of the E. and W. points, and culminating some distance below the pole star. This continued to rise until 8 P. M., when its apex was within a few degrees of the pole.

At $9^h\ 20^m$ a low luminous segment showed itself on the horizon beneath the arch. The latter now resolved itself into an array of bright streamers, with equidistant shadowy spaces between them.

At 9^h 30^m the streamers had extended and grown brighter, while the low luminous segment, diffusing itself upward, had merged into the outer arch, which now reached nearly to the pole star. At this moment the arch began to send off successive waves of light, rapidly following one another towards and beyond the zenith. In a few seconds this wave movement gave place to more rapid and seemingly broken pulsations, flitting upwards in close succession through the northern, eastern and western quarters of the sky, and visible, though less distinctly, in the south. This wonderful appearance exhibited everywhere a convergency of the lines of motion towards a point considerably south of the zenith.

When these luminous phenomena were at their height, every spot to which the eye was directed, except the southern quarter near the horizon, was traversed by quickly successive flashes of white, greenish, and pale roseate light, all seemingly moving upwards.

At 10^h 30^m the pulsating movement again extended over all the northern and part of the southern half of the sky. Innumerable waves of white, yellowish and purplish light chased each other from every quarter towards the magnetic pole, while the crimson flush spread wider and higher from the west.

The various phases of this aurora recurred according to a somewhat uniform order of succession. First, the dark segment on the northern horizon took a regular arched form, and as it rose, became bounded above by a broad luminous curve, at the same time developing one or more bright concentric arches within. The streamers now shot forth from all parts of the luminous zone; and as these increased the upper arch faded away, as if it had expended itself in producing them. And now the lower arch took its place, to be obliterated in its turn by a like seeming process of exhaustion. At length, one of the grander effusions of light coming on, the whole arch was broken up, and the dark segment below was reduced to a shapeless mass. Then there occurred a comparative pause in the phenomena, until the dark segment again took form, with its one or more luminous bands, and a like cycle of development was repeated.

9. *Observations at Steubenville, Ohio,* (lat. 40° 25′), *from the Steubenville Daily Journal.*

The magnificent auroral display of Aug. 28th was unusually interesting. 1st. It covered a much larger space of the heavens than any we ever saw before, at least since 1835. 2d. It lasted from dark until daylight, appearing with the first approach of darkness and only disappearing as daylight gradually overpowered it. 3d. Instead of an arch, shooting up rapid and various colored rays, its first appearance was that of a luminous mist,

with barely perceptible rays along its southern border, and moving with the rolling motion of clouds, rather than the straight darting motion usually seen in auroras. 4th. It varied in intensity more than any we have ever seen before, twice fading nearly out, and remaining so for nearly half an hour or more, and then kindling up with greater brilliancy than before.

About 7½ P. M. it was a barely perceptible light in the northeast. As the darkness deepened, this luminous spot grew brighter, and moved to the south, till a little before 8 P. M. when the light spreading from it met that coming from the west, and formed an arch about half way between the zenith and southern horizon, and there its advance ended, and it began instantly fading out. It retreated just as it had advanced, only more rapidly, and at $8^h\ 10^m$ there were left only the two centres in the northeast and northwest with a fitful gleam between them. During this retreat, portions of the luminous cloud broke off and floated for some minutes far away from the main body, surrounded by deep darkness, like islands. One of them, and the most beautiful, was a long bright bar in the south, which extended more than half way across the sky from west to east, with a wide sea of darkness between it and the parent cloud, which gradually melted away and disappeared to the westward.

At 9 P. M. the light advanced again, this time with a blood-red tinge in the eastern and western portions, and passed clear to the south as before, but shooting up many and variously colored rays, sometimes from the east, sometimes from the west, sometimes from the north, and from all parts of an irregular luminous arch that bent over the northern horizon about twenty degrees above it. This display faded away in an hour, and at 10½ P. M. there was no light that would attract attention, more than is frequently seen in the north.

About 3 A. M. it blazed up with redoubled brilliancy, shooting up white rays far above the zenith, and making the earth as light as a full moon behind a mist could have done. This time the rays seemed to dart up in broad masses, giving the sky the appearance of being covered with slabs of light, which were tinged with red in the zenith, and rested on a broken irregular arch in the north that in some places fell to the horizon, and in others rose in angular openings to thirty degrees above. During this last display, the pulsations of the aurora were beautifully marked, the rays shooting up in a sort of volley, many hundreds together; while broken and separate masses of luminous cloud were seen in various parts of the sky.

10. *Observations at Burlington, N. J.*, (lat. 40° 5′), *by* BENJAMIN V. MARSH.

Aug. 28th an arch of light rose in the north, passed the zenith and descended to within about 20° of the south horizon by 8^h 30^m P. M. Soon after this, the whole space overhead was occupied by a dense unbroken cloud of milky whiteness. There was however up to this time a considerable number of small black clouds moving southward, which soon afterward disappeared entirely. These clouds were very thin, and we were for a while in doubt whether they were not patches of clear sky; but by watching their effect upon the stars, we satisfied ourselves that they were clouds.

Still later, about 20° above the south horizon, there was a dense whitish arch a few degrees in width, its lower margin being regular and well-defined. About 30° or 35° above the north horizon was the top of another arch, wider than the first, but not so regular or well-defined. Between these two arches were numerous streams and fragments of white auroral cloud.

Between $9\frac{1}{2}$ and $9\frac{3}{4}$ P. M. there was a perfect corona; the streamers on the south side were short, and mostly white, and moved pretty rapidly westward. Their number at one time was probably five or six. At one time the central space was perfectly clear; but afterwards the streamers ran through it to its centre.

11. *Observations at Crawfordsville, Indiana*, (lat. 40° 3′), *by* Prof. JOHN L. CAMPBELL.

Aug. 28th, the aurora began about 7^h 30^m P. M. with an unusual white light in the form of an arch in the north. At 8^h 45^m P. M. the white light appeared in two brilliant spots about 60° on each side of the magnetic pole.

At 9 P. M. streamers of white, red and pink light in circular currents about the magnetic pole (variation 5° 45′ east) extending beyond the zenith.

At $9\frac{1}{4}$ P. M. the streamers were concentrated into brilliant ones passing nearly along the magnetic prime vertical.

At 10 P. M. streams of white light were formed in the east, and rapidly passed westward, a little south of the zenith. These streams or clouds were entirely separate from each other, and the more northern band, and possessed a real motion. The time occupied in passing was about one second. Not less than twenty flashes passed over. They were formed about 30° above the eastern horizon, and disappeared about 60° above the western. After 10 o'clock the white light in the north became more brilliant, and tinged with red, extended very far towards the south. We traced the red tinge on the east to within 40° of the

south point, and on the west to within 50° of the same. At 11 P. M. the aurora was still bright in the north.

Aug. 29th, 2 A. M. Very brilliant streams of white and red light filled the northern hemisphere. These streams were perpendicular to the horizon in the north, and were inclined at regular decreasing angles towards the east and west to about 70°. Brilliant flashes passed across the heavens, originating in the northeast, and passing in a southerly direction vanished in the southeast.

At $2^h\ 45^m$ A. M. Heavy bank of red light ten degrees north of east. Patches of white light in north with occasional streams.

At 3 A. M. the whole northern hemisphere was filled with streams of white light with the same inclination as at 2 A. M. At $3^h\ 15^m$ A. M. the auroral storm was at its height. Flashes of red and white light each instant flew across the northern hemisphere. At $3^h\ 30^m$ A. M., a bright band of white light covered the hemisphere except low down in the north; and the incessant flashes in the northeast and east still continued. Long streams of light flashed across the entire hemisphere. The lower part of the band passed through the heavens at an elevation of 40°. In the zenith was displayed a brilliant red bank. In the east, red and white flashes were very brilliant—better defined but not so rapid in transition as at $3^h\ 15^m$ A. M.

At $3^h\ 45^m$ A. M. Magnificent corona in the zenith. Central portion spiral, red and white, changing instantly to a beautiful rose color, with spiral streams shooting forth into all parts of the heavens; the most brilliant streams flowing east and west. The heavens were completely covered with these streams of light.

At 4 A. M. the white light in the north was still very bright, but the dawn obscured the eastern bank.

12. *Observations at Philadelphia,* (lat. 39° 57′), *by* CHARLES J. ALLEN.

Soon after half past 8, Aug. 28th, the southern margin of the luminous auroral curtain was well defined, and its position between two fixed objects carefully noted. It was afterwards ascertained by actual measurement that this gave an elevation of about 22½ degrees above the southern horizon.

13. *Observations at Sandy Spring, Md.,* (lat. 39° 9′), *by* Prof. BENJAMIN HALLOWELL.

The elevation of the southern margin of the luminous auroral curtain above the southern horizon, Aug. 28th, about 9 P. M., I thought, and mentioned to those with me, was about the meridian altitude of the equator, say 51 degrees.

14. *Observations at Stockton, California,* (lat. 38° 10′), *from the San Joaquin Republican.*

The aurora of Aug. 28th first appeared about 9 P. M., when a faint white light commenced about north and extended to about east by north. About 9¼ P. M. great streams of red and blue shot up all along the northeastern horizon, but they appeared to shoot highest about mid-way of the light. These streams would faint and brighten in such a wonderful manner, that we imagined some painter in the skies drew his great brush from the horizon up to 40 or 50 degrees, dipped with vermillion, then with sky blue, and then with white and flesh color.

15. *Observations at Sacramento, California,* (lat. 38° 34′), *by* Thomas M. Logan, M.D.

I have observed the aurora only at five different times during a residence of nine years at Sacramento, viz., Dec. 16, 1857, Oct. 27, 1858, Aug. 28, 1859, Sept. 1, 1859, and Oct. 18, 1859. I know of but three other well authenticated instances of the phenomenon having been witnessed in California; one by Geo. H. Goddard at Sonora, Jan. 19th, 1852; and two by Henry Gibbons, M.D., at San Francisco, Jan. 19th, 1852, and Feb. 19th, 1852. This shows the infrequency of its appearance in this State.

The aurora of Aug. 28th, 1859, commenced at 9 P. M. and ended about 3 A. M. next morning. The appearances exhibited during this extended period were so various as to render it impossible to note the particular hours of the different changes. In its perpetual movements and fantastic changes were recognized all of the characteristic features that mark this phenomenon, from its close resemblance to the aspect of the sky before sunrise, to the formation of the luminous arc, darting forth palpitating rays towards the zenith, of white, pale red, and deep blood color. This last mentioned feature was seen in its greatest glory at about midnight; and lambent streamers about this time were noticed to shift gradually from west to east, and vice versa. The summit of the arc was not more than six or eight degrees above the horizon, and appeared to coincide with the magnetic meridian. The lower segment of the arc was not as obscure as in the other auroras observed by us. The most remarkable feature during the whole display, was the long continued gleaming of a dark rose or carmine illumination, particularly at the western extremity of the arc; this rosy light passing occasionally along the belt with a fluctuating movement towards the opposite end. The whole northern sky at one time seemed to be a cupola on fire, supported by columns of divers colors, relieved and intensified by dark shadows or rather streaks. The sky remained almost entirely clear the whole night. This au-

rora was observed in all parts of the State, and very generally throughout the whole north Pacific region.

The aurora of Sept. 1, 1859, was first observed about 10 P. M. There was seen first a warm glow in the northwest, and two white silvery clouds in the north. Soon the light extended in all directions, until the entire firmament was suffused with a ruddy light so bright at times that the hour could be distinguished on the dial of a watch. At midnight a splendid glowing corona was seen extending from the eastern to the western horizon, and the whole southern hemisphere appeared to be in one continuous blaze. These ever changing phenomena continued to manifest themselves until lost in the dawning day.

16. *Observations at St. Louis, Mo.* (lat. 38° 37′), *from a St. Louis Journal.*

On the night of Sept. 1st we had a most beautiful exhibition of the aurora. The view did not approach its highest grandeur until after 11 o'clock. At first there was a hazy appearance, embellished here and there by faint streaks and tremulous touches of light. Then the wavy pencillings grew stronger and broader, and the light spread until it had crept up to the zenith, when half of the world seemed enveloped in a sheet of mellow flame.

17. *Observations at Louisville, Ky.* (lat. 38° 3′), *from the Louisville Journal.*

One of the most magnificent auroras ever witnessed in this latitude was seen about 9 o'clock, Aug. 28th. The whole heavens, from the northern horizon to the zenith, were brilliantly illuminated with a rose-colored light, and the flashes were very vivid. The northwestern sky was the portion most brilliantly illuminated, but in the northeast, the rosy flush was exquisitely beautiful.

18. *Observations at Charleston, S. C.* (lat. 32° 46′), *from the Charleston Mercury.*

Aug. 28. The northern heavens were brilliantly lighted until about 2 o'clock Monday morning, when the aurora faded entirely away. On the morning of Sept. 2d the auroral exhibition far surpassed any former instance observed in this city, for the general extent and diffusion of the lights.

19. *Observations at Bermuda* (lat. 32° 34′), *from the Bermuda Royal Gazette.*

The aurora of Aug. 28th appeared to ascend from a few degrees above the northern horizon a great distance upwards towards the zenith, assuming a variety of shades and beautiful

colors; sometimes in tremulous sheets of pale yellow, changing gradually into a deep crimson, or shooting upwards in streams of light resembling those frequently observed from the setting sun. It covered at times the entire space between N.W. and N.E., leaving the sky from the horizon to its apparent base, perfectly clear. Towards 10 o'clock its brilliancy gradually died away, but it continued more or less visible till the dawn of day.

Sept. 2d, between 2 and 3 A. M., the aurora displayed itself in greater splendor than it did on the 28th. Many persons were awakened from their slumbers by the intense light which entered their chambers.

20. *Observations at Savannah, Ga.* (lat. 32° 5′), *from the Savannah Republican.*

On the evening of Aug. 28th we had a brilliant display of the aurora borealis. The northern sky, for an extent of some forty-five degrees, was luminous with a mass of red light, from whence shot up towards the zenith the usual streaks, at times vivid and beautiful.

Sept. 2d, about 1 A. M. the aurora again appeared and was of a very intense and beautiful color, being a mixture of pink, gold and purple. After it had reached an elevation of about 45°, it seemed to dissolve in the centre, and spread out both east and west. About 2 o'clock it formed a complete arch overhead, from N.E. to S.W. About 3 A. M. it gathered in the zenith, and sent out bright fiery flashes in every direction. It was far more magnificent than the aurora of Aug. 28th.

21. *Observations at Mobile, Ala.* (lat. 30° 41′), *from the Mobile Daily Register.*

The aurora showed itself a little east of north about 7¼ P. M. Aug. 28th, and kept up the exhibition until about 9½ P. M., when its paling light died out. It was of a reddish hue, inclining to yellow, and its flickering light assumed a kind of pyramidal form, shooting up into the heavens, nearly to the zenith. Then the centre seemed to grow dim, and a division took place, its right wing moving to the extreme north, where the left wing after a short time joined it.

Between 12 and 3 o'clock on the morning of Sept. 2d, the aurora was repeated upon a scale of beauty and grandeur never before witnessed in the south. A bright pink colored light shot up from the northern horizon, and darted off into beautiful rays, flickering and brightening until they reached the zenith, and soon encircled the hemisphere like a belt from east to west. After about three-quarters of an hour, during which time the aurora occasionally furnished light enough to read by, the bright and beautiful light suddenly clothed the entire firmament.

22. *Observations at New Orleans, La.* (lat. 29° 57′), *from the New Orleans Daily Delta.*

A grand auroral display appeared between 8 and 9 o'clock, Aug. 28th, in the northern horizon. A column of light first shot up into the sky, which soon spread up towards the zenith, and around the horizon, and made one of the most magnificent appearances that the sky has ever exhibited.

About 11 P. M., Sept. 1st, the aurora reappeared and continued until 3 or 4 o'clock in the morning. Nearly the whole visible northern hemisphere was covered with a fiery, blood-reddish, though transparent vapor. The deepest color was on the east and west, a space around the polar centre seeming to be the only non-illumined portion of the northern heavens. Over this crimson ground, spears and pencils of pale flickering light shot up at intervals from the horizon, converging at a point near the zenith. The whole sky along those lines was at once luminous and tremulous. In a moment vast segments of arches would rise, and then suddenly disappear.

23. *Observations at Galveston, Texas* (lat. 29° 17′), *by* Prof. C. G. FORSHEY.

Aug. 28th, as early as twilight closed, the northern sky was slightly lurid, and at times lighter than other portions of the heavens. At $7^h\ 30^m$ a few streamers showed themselves. Soon the whole sky, from Ursa Major to the zodiac in the east, was occupied by the streams or spiral columns that rose from the horizon. Spread over the same extent, was an exquisite roseate tint which faded and returned. Stately columns of light reaching up about 45° from the horizon, moved westward about one degree for every ninety seconds of time. There were frequent flashes of lightning, apparently from distant clouds, along the whole extent of the aurora; but no clouds were visible, except a single streak near the horizon. At 9 P. M. the whole of the streaking had faded, leaving only a sort of twilight over the northern sky, and we ceased our observations.

At 3 A. M. Aug. 29th, I awoke and perceiving that it was very light outside, rose, and found the whole northern heavens again on fire. Such a display I have never seen equalled since the aurora of Sept. 1, 1839. The whole distance before-named was tinted with the roseate hue; darker, nearly crimsoned at the two flanks. In the centre, near the meridian, stood a stupendous pyramid of white light with its apex near the zenith. On either side at some twenty degrees, stood a pyramid of rosy light, each about sixty degrees in height and in exactly symmetrical positions. Scarcely had I sketched the outline of this noble spectacle when the columns drifted westward and faded.

Another fine display of the aurora commenced about 10½ P. M. Sept. 1st, and lasted until near daylight the next morning. A dusky red, like the reflection of an immense conflagration, overspread almost the entire heavens, beyond the zenith, far down towards the southern horizon.

24. *Observations at Sea* (lat. 28° 30′, long. 79° 30′), *Barque Pride of the Sea.*

Sept. 2d, at 12^h 35^m A. M., a bright spot or cloud appeared in the N.W. which shot up rays resembling the aurora, and in thirty or forty minutes formed an arch across the horizon from N.W. to N.E., which became lighter as it arose, and at 1^h 15^m A. M. it was light enough to read the smallest print without a light. At the time the horizon was cloudy, but overhead was clear, the larger stars being just seen. At 2^h 15^m the arch passed over to the southward, when it became dark again.

25. *Observations at Key West* (lat. 24° 32′), *from a Journal.*

A brilliant exhibition of the aurora was witnessed at this place Aug. 28th, and a still more brilliant one on the morning of Sept. 2d. The whole northern half of the heavens was tinged with crimson, red as blood. Occasional flashes of blue and white light shot up towards the zenith and then slowly melted away.

26. *Observations at Havanna, Cuba* (lat. 23° 9′), *by* M. Andreas Poey.

In his former communication (Am. Jour., vol. xxviii, p. 406) Mr. Poey stated that during the auroras of Aug. 28th and Sept. 1st he was unable to obtain any indications of atmospheric electricity. In a later communication he states that neither at the time of these auroras, nor on the preceding or following days, was there the smallest interruption or disturbance experienced on the electro-magnetic telegraph lines of Cuba.

27. *Observations at Inagua, Bahama Islands* (lat. 21° 18′), *from the New York Journal of Commerce.*

The aurora of Aug. 28th was distinctly seen from this place, and was supposed to have been a large fire in the neighborhood. It was remarkably brilliant, but was not attended by that flashing appearance which is sometimes noticed in higher latitudes.

28. *Observations at Cohe, Cuba* (lat. 20°), *by* George F. Allen.

On the night of Sept. 1st a Spanish mechanic who worked for me called me out of bed to see the great light in the northern sky. He was much struck with it, and said the people in St. Jago de Cuba would think the end of the world was at hand.

I found a display which would have been considered more than ordinary even in the latitude of New York. It resembled the auroral displays occasionally seen in New York when more than usually brilliant. The same rosy light, on a darker horizon, fading off into yellower and whiter as it spread upwards, variegated occasionally with white streamers. It extended horizontally, according to my rough estimate, about one-third or two-fifths of the horizon, and upwards about two-fifths of the arch of the visible heavens. It was a very brilliant display, and surprised me much by its brilliancy in that latitude.

29. *Observations at Kingston, Jamaica* (lat. 17° 58′), *from the New York Herald.*

An extraordinary light appeared in the north on the night of Sept. 1st and the morning of Sept. 2d. It appeared as if there was a colossal fire on earth which reflected its flames on the heavens. The whole island was illuminated. The light was seen at Montego Bay (lat. 18° 21′) at 10 P. M., but it was not observed at Kingston until 1 A. M. Sept. 2. It continued until 5 A. M., when it gradually disappeared. It looked as if Cuba was on fire, and many believe that a portion of this island had been destroyed by a conflagration. Other persons were of opinion that the light was that of an aurora, but the aurora has never before been seen in this latitude. A similar fire was observed on the north side of Jamaica Aug. 28th.

30. *Observations at Guadeloupe, West Indies* (lat. 16° 12′), *from L'Institut.*

On the 2d of September, from $1\frac{1}{2}^{h}$ till daylight, an Aurora Borealis was seen at Guadeloupe to the great astonishment of the population. Its ruddy light was noticeable in the interior of the houses. At the centre of this vast conflagration were noticed two rays of whitish light which rose parallel to each other, passing a little to the left of the pole star. The aurora attained its maximum of brightness at 3 A. M.

31. *Observations at La Union, San Salvador* (lat. 13° 18′), *from the Gaceta del Estado.*

On the night of Sept. 2d, a most extraordinary phenomenon was witnessed. About 10 o'clock, a red light illuminated all the space from north to west, to an elevation of about 30° above the horizon. The light was equal to that of day-break, but was not sufficient to eclipse the light of the stars. The sea reflected the color, and appeared as if of blood. This lasted until three in the morning, when a dense black cloud arose in the east, and commenced to spread over the colored portion of the heavens,

presenting a most curious spectacle; for in the parts where the cloud was not dense enough, the red light shone through, and formed a thousand fantastic figures, as if painted with fire on a black ground.

In the city of Salvador (lat. 13° 44′) the same phenomenon was visible, occupying the same space in the heavens, and the red light was so vivid that the roofs of the houses and the leaves of the trees appeared as if covered with blood.

February, 1860.

ART. XXV.—*Correspondence of Mr. Jerome Nicklès, dated Nancy, November 10th*, 1859.

Biography.—Cagniard-Latour.—We have already given some notice of this physicist, so lately lost to science; the following details are taken from an autobiography, which gives a very interesting account of the circumstances which led him to some of his discoveries. His researches may be classed under four heads; acoustics, mechanics, chemistry, and general physics having successively occupied his attention. His first invention (1809) was a pneumatic Archimidean screw, which is now in common use for conveying gases under liquids, and has received the name of the *Cagniardelle*. The ingenious inventor simply inverted the action of the ordinary screw of Archimedes, making it revolve from right to left. As Arago remarked in the chamber of deputies in 1844, during a discussion of the law of patents, although the *Cagniardelle* is nothing more than Archimedes' screw reversed, it is not less true that 2000 years had passed before any one conceived the idea of making this simple change and rendering it available in mechanics as a pneumatic machine.

The *Siren* (1819) as is well known, is an instrument for measuring the vibrations of the air which constitute sound. If, as had been supposed by physicists, the sounds produced by musical instruments are due to the regular succession of impulses given to the air by their vibrations, it was evident that a mechanism which would enable us to strike the air with the same rapidity and regularity should in like manner produce sounds. Reasoning in this manner, Latour was led to the invention of this well-known and beautiful instrument.

In 1822 he published his experiments on the combined action of heat and pressure upon certain liquids, such as water, alcohol, ether and naphtha. He imagined that the dilation of a volatile liquid must have a limit beyond which, notwithstanding the compression, it would pass to the state of vapor, provided that the capacity of the vessel permitted the liquid to expand beyond its maximum of dilation. The remarkable results to which he was led by this reasoning are well known.

In 1837 he published with Mr. Demonferrand the description of an acoustic pyrometer, by which the authors proposed to render the measurement of all temperatures appreciable through the medium of sound. In the same year he examined the pressure to which the air in the trachea is exposed during the act of sounding. He had previously been employed

in investigating the pressure to which the air in the lungs is exposed when employed in sounding certain reed instruments, and had found it for the clarionette equal to the pressure of the atmosphere, *plus* a column of water of 30 centimeters. In order to extend these inquiries to the human larynx, it was necessary to find a person having an opening in the trachea, and yet able to produce vocal sounds at will. After long search, Cagniard-Latour found such a man, who was for his purpose as precious as the subject with the permanent gastric fistula became for the well-known experiments of Dr. Beaumont. In the same year he made known his chronometric balance, designed to measure the dynamic effects of machines in motion.

Next appeared a memoir on the alcoholic fermentation, of which these were the principal results:

1st. The yeast of beer is made up of little globular bodies, apparently vegetable, and capable of reproduction in two different manners. These bodies seem to act upon a solution of sugar only when in a state of life, and he hence conceived it probable that it is by a vital process that they transform the sugar into carbonic acid and alcohol. This investigation was suggested by a question long before proposed as the subject of a prize by the French Academy of Sciences, in the seventh year of the Republic, viz. What are the characters which distinguish among animal and vegetable matters, those which serve as ferments and those which are subject to fermentation?

Cagniard-Latour now resumed his researches upon vibrating bodies, and succeeded in producing a sound by causing a glass rod to oscillate easily between two metallic columns. The peculiarity in the sound thus obtained was that the number of vibrations indicated by it corresponded only to one-half the synchronous number of simple oscillations of the rod, although the apparatus was arranged in such a manner that each movement backwards and forwards should produce two strokes of equal intensity, by the alternate blows upon the two columns. The experiments made with this instrument enabled him to give the theory of the production of sounds by vibrating cords. During the same year (1840) he studied the production of grave tones, like those of the human voice, and made various researches to discover the mechanism of the human voice.

In 1851 he laid before the Academy a memoir upon the *moulinet à battemens*, demonstrating some new acoustic phenomena. In two previous papers published in 1830 and 1831, upon the sounds produced by solid bodies turning with great velocity, he had shown certain facts relative to the musical tones produced by the friction of the axle of a wheel against its supports. Subsequently he conceived the idea that a solid of revolution, a cylinder for example, arranged so as to turn vertically around its axis upon two center-holes, might give rise to pulsating sounds (beatings), although moving with a feeble velocity, provided it received in the lower center-hole the friction of the revolving axis of a winch turning in an opposite direction to the cylinder. It was to the instruments constructed according to these ideas that he gave the name of the *moulinet à battemens*.

We have also from Cagniard-Latour an investigation on the action of heat on different kinds of wood enclosed in hermetically sealed glass

tubes. Sir James Hall, in his experiments upon the saw-dust of pine wood and of horn in sealed gun-barrels, had observed that the mixture underwent fusion and was cemented into a sort of coal. Similar results were obtained by Latour with thick glass tubes.

These are not the only researches which we owe to this lamented physicist; in our previous letter we mentioned that while connected with the Government powder works in 1814, he made some useful improvements in that department, especially in the glazing of powder. We also spoke of his very light, portable, and efficient flour-mill, which consisted of a steel rasp moving vertically with an alternating motion between two fixed rasps, also of steel. During the severe winter of 1816, the streams being all frozen and the mills stopped, Cagniard-Latour was directed to have an immense number of these hand-mills constructed, and thus in a few days the public were saved from the fears of a famine. We have also seen the part which he took in the establishment of gas-lighting in Paris. He was besides the constructer of an aqueduct, a model of its kind, suspended between two rocks, and formed of a single span 200 metres in length. When we consider these varied achievements, we learn with surprise that it was only in 1851 that he became a member of the Academy of Sciences.

The aurora borealis and its theory.—The late brilliant auroras have called attention to De la Rive's theory, of which we have formerly spoken, and which is explained at length in his *Traite d'Electricité*. Great perturbations were observed along the telegraphic lines over the European continent, similar to those remarked some years since by Matteucci in Tuscany, and Highton in England. The most remarkable fact in these electrical disturbances is that they were produced by a continuous current, while those of a thunder storm are instantaneous, and only mark points upon the paper in Morse's apparatus; the aurora of the 29th August traced continuous lines of greater or less length. These effects lasted for several days after the aurora.*

It is fortunate that the aurora of the 29th was carefully studied by a man so competent as Coulvier-Gravier. This observer, who has studied the heavens for nearly 60 years, and has so much advanced the sciences of Cosmography and Meteorology, was found that night as usual at his post in the observatory which the government prepared for him twenty years since at the Luxembourg palace. The phenomenon was in all its splendor at $2^h\ 45^m$ A. M.; its extent included more than 100°, and M. Coulvier-Gravier declares that he had never seen it more beautiful during the long period of his observations.

The observations during the late auroras support the theory of De la Rive, which he has thus defined. The vapors constantly rising from the sea, and especially from the equatorial regions, carry with them to the higher regions of the air a great quantity of positive electricity, to which they serve as the vehicle, leaving the surface of the globe negatively electric. Borne to the poles by the currents which always prevail in the higher regions of the atmosphere, these vapors carry with them their electricity, and thus give to the whole atmosphere a positive electric condition, which diminishes from above downwards. This positive electricity tends unceasingly to combine with the negative electricity of the earth,

* See p. 92, this volume.

directly through the stratum of air, but more especially at the two poles, where the currents of vapor carried by winds converge and are condensed.

According to Mr. De la Rive the aurora of the 29th August was a natural consequence of the great drought which had prevailed over the continent. The dryness of the air had prevented the positive electricity from neutralizing itself directly with the negative of the earth. From this accumulation of electricity there finally took place towards the polar regions a discharge much more intense and much more rapid than usual, which constituted the brilliant aurora in question. A fact which tends to show that the aurora is an electrical and not a magnetical phenomenon, is furnished by the ozonometrical observations made by Mr. Perigny, at Versailles. From the 28th of August to the 2d of September, he found the air to contain a quantity of ozone, relatively large, and more abundant by night than by day.

Human Remains in the Drift.—For the last twenty years it has been known that axes of flint, evidently wrought by human skill, were found in beds of the drift at Amiens, associated with the bones of extinct species of animals. This discovery, made by a learned antiquary, Mr. Boucher de Perther, had been regarded as doubtful, and it was supposed that sufficient precaution had not been observed in conducting the explorations. Recent discoveries made in a cavern at Brixham, near Torquay, in England, however, recalled attention to the observations of Mr. Perther, and Mr. Prestwich, with several other geologists, accordingly visited Amiens in order to make the excavations necessary to decide this important question. Every precaution was of course taken to prevent errors or deception, and Mr. Prestwich could find nothing at Abbeville, but at Amiens he was more fortunate; one of his companions, Mr. Flower, in examining a bed of gravel at six meters from the surface, and evidently undisturbed, extracted with his own hands a fine axe, more than five inches long. After this Mr. Prestwich having been informed that a similar discovery had been made in 1737, at Haxne, in Suffolk, visited the place, and learned that some years since wrought flints were still found in abundance, although rare at present. He however succeeded in finding two axes, similar to those of Amiens, but of less perfect finish. An analogous fact has just been verified by French geologists, who have found these axes in Picardy, associated with remains of *Elephas primogenius*, *Rhinoceros tichorhinus*, *Equus fossilus*, and an extinct species of *Bos*. The conclusion from all these facts is that man was cotemporaneous with these several species of large animals now lost, and known to us only by their fossil remains.

Curare in the treatment of Tetanus.—This important question has elicited much discussion at the Academy, and many contradictions and differences of opinion. In 1850, Mr. Claude Bernard showed that the curare, or arrow poison, acts by paralyzing the system of motor nerves; following up this observation, an Italian physician, Villa, of Turin, made in 1854 a series of experiments, in the course of which he showed that curare exerts upon the nervous system an action so completely antagonistic to that of strychnine, that the two poisons may be neutralized by each other. Dr. Villa having been attached to the French hospitals during the late war, was induced to apply the curare in the treatment of three cases of trau-

matic tetanus one of which recovered. It was the case of a soldier wounded by a ball in the right foot. The curare dissolved in water was applied to the wound, with the effect of diminishing the pain and suspending temporarily the tetanic spasms, which, however, returned. After fifteen days of this treatment the patient left the hospital completely cured. The experiments of Mr. Villa have been repeated in the hospitals of Paris; but as yet only a single case of cure has been reported out of several failures; the experiments, however, continue. According to a letter from Sir Benj. Brodie, of London, to the Academy of Sciences, the application of curare as a specific against tetanus, was unsuccessfully made upon horses at London in 1815, by Dr. Sewell, professor at the Veterinary College. Such are the principal facts in the question as far as yet made known.*

The new alloys of Platinum.—We recall the interesting researches of Messrs. Deville and Debray on this subject only to mention their industrial applications. Hitherto it had been supposed that the presence of iridium impaired the quality of platinum, but the labors of Deville and Debray have shown that on the contrary alloys of these two metals may be prepared which are greatly superior to pure platinum, presenting greater strength and rigidity, and resisting better both heat and acids. Thus the alloy containing 21·3 of iridium is highly malleable and scarcely attacked by aqua regia. As the quantity of iridium is less, the alloy becomes softer, and one containing 10 or 15 per cent. is peculiarly fitted for chemical vessels. These alloys are now largely wrought in Paris; retorts for the manufacturers of sulphuric acid have been made, having the strength and rigidity of rolled iron.

Messrs. Deville and Debray are at present making some trials at the French mint, for the Russian government, to determine the fitness of the new alloys for coinage. They have found that those containing 20, 10, 7½ and 4 per cent of iridium, take the impression of the dies with great perfection. The same is true of the natural alloy, which is obtained by directly fusing crude platinum, and retains only the iridium and rhodium in combination with the platinum, the other metals having been removed by volatilization or oxydation. The platinum workers of Paris are now manufacturing and selling the new alloys, and, contrary to the wishes of the discoverers, are exacting higher prices than for pure platinum.

Rifled Cannon.—The invention of these guns appears to be due to a former captain of artillery, Mr. Tamisier, who was in 1842 charged with the course of instruction in musketry at Vincennes, where he applied himself with great assiduity to the study of various questions connected with his profession, with results which have contributed very much to the improvement of the system of musketry instruction in the army and in the arms of precision. After studying the effects of elongated projectiles in rifles and muskets, Mr. Tamisier was led to construct a rifled mortar, with cylindro-conical shells. The duke of Montpensier, then colonel of artillery, at once saw the importance of this new project, and after causing many experiments to be made, ordered at his own expense, in 1847, the construction by Capt. Tamisier of elongated balls and shells. These however were not tried until 1850, from which time up to 1853, Mr.

* See experiments on two new varieties of South American arrow poison by Drs. Hammond and Mitchell, Am. Jour. Med. Sci., July, 1859, (this Jour. [2], xxviii, 303).

Tamisier continued his experiments at Vincennes and at La Fère. The first experiments were made at Vincennes, on the 15th of July, 1850, when it was shown that rifled six-pounders with elongated projectiles, carried much farther and with greater exactness than ordinary guns of smooth bore. On the 14th August, 1851, Capt. Tamisier repeated his experiments at the polygon at Vincennes, before the President and the Minister of War. A six-pounder rifled with three grooves projected balls of five kilogrammes to a distance of 1500 meters with a charge of only 700 grammes of powder. The government then ordered further experiments to be made, which were conducted at La Fère, a fortified place in the Department of the Aisne, where greater secrecy could be secured than near Paris. The trials were there made by Col. Trenille and by Col. Virlet, now directors of the School of Artillery at Metz, and led to a complete solution of the problem, so that the army of Italy was able to bring into the field more than 200 rifled guns of a calibre of 84 millimeters, requiring for service and transport only two-thirds the men and horses hitherto necessary, and carrying balls of four kilogrammes 3500 meters with such precision that at this distance they would all fall in a rectangle of 80 meters in length by 40 meters in breadth.

Acclimatation.—In our last letter we spoke of a number of Arabian camels, which the Society of Acclimatation had procured in Algeria, to be introduced into Brazil. We learn that they were safely landed there after a voyage of 28 days.

Photo-Chemical Researches: Persistent Activity of Light.—In this Journal for March, 1859, p. 257, we have described the beautiful experiments of Niepce de St. Victor, upon the persistence of the effects of insolation. According to Mr. Laborde, the curious results obtained by Mr. Niepce are due not to radiation, but to a veritable emanation from the card-board, which has been impregnated with tartaric acid, and exposed to the sun-light. The principle subsequently evolved from this card-board, which acts upon sensitive paper prepared with a silver salt, is, according to Mr. Laborde, no other than formic acid. This is well known to be a product of the oxydation of tartaric acid, especially when in contact with peroxyd of lead. He found that a concentrated solution of tartaric acid mixed with peroxyd of lead, and placed in a dark place, evolved vapors which reddened litmus and discolored prepared paper after a very short time. There is, then, in the case of insolated tartaric acid, an emanation and not a radiation; but Mr. Niepce obtained similar results with nitrate of uranium spread upon porcelain and insolated, in which case it is not easy to conceive of an emanation of vapor. However this may be, the facts announced by Mr. Niepce remain, and the only difference of opinion is as to their interpretation.

Maritime Canals.—Besides the canals proposed at Panama and Suez, the question of one between the Caspian and the Black Sea is now being discussed in Russia. The construction of such a canal was ordered by Peter the Great, after having been commenced a century before under the Sultan Selim II, upon the suggestion of his Grand Vizier, Mohammed Sokolli, who was for Turkey what Colbert was for France. The canal was commenced in 1569, and during two years there were employed upon it 5000 janissaries and 20,000 prisoners.

The canal now projected will unite the Don and the Volga, the first of these falling into the sea of Azof, and the second into the Caspian, near Astrachan, at a point designated on the maps as Tzaritsin. The two rivers are separated by a distance of only 55 kilometers. The importance of a canal which will connect the Caspian with the Mediterranean will readily be seen, and it must at the same time be confessed that it will be much more easily executed than the tunnel under the channel proposed by Mr. Thornè de Gamond, since the canal, being exclusively on Russian soil, will be a question for engineers, with which politicians will have no concern.

For the last thirty years, it has been proposed to make a sea-port of Paris by a ship-canal leading from the Channel; the introduction of railways caused the project to be for a time abandoned, but it is now again discussed. Among the plans proposed is one which appears in the *Ann. des Sciences* of the 8th October, 1859. It is intended to excavate at the mouth of the Seine, near Havre, a harbor 1000 meters long and 200 wide, with a depth of 12 meters. The canal, 150 kilometers in length, will follow the course of the Seine, and will have the same depth as the harbor, which will be furnished with gates to preserve the water at a proper height. A double line of railway will accompany the canal, one line serving for passengers and freight, and the other for towing vessels, which will thus make the voyage in four hours.

The position and direction of the canal will be such that the west winds, which are the most frequent at Paris, will help vessels in coming up, while the water of the Seine, to be let in by sluices, will aid them, by their current, in descending. The great difficulty in this enterprise will arise from the tunnels required, the total length of which will be from twenty to twenty-five kilometers. These will be vaulted, and with a height of 30 meters by at least 50 meters in breadth. Mr. Piorry estimates that the expense of this work may amount to a billion (1,000,000,000) of francs.

Engineers are also occupied with a plan for joining the English Channel with the Mediterranean, by taking advantage of the river Rhone, the Saône, the Yonne, and the Seine. The details of its execution having only a local interest, we spare our readers their recital.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On two new series of Organic Acids.*—Hentz has studied the action of methylate of soda and similar substances upon chloracetic acid, and has obtained interesting new acids in which hydrogen may be considered as replaced by the deutoxyds of methyl, ethyl, etc. The acid resulting from the action of methylate of soda on chloracetic acid has the formula $C_6H_6O_6$, which is that of lactic and paralactic acids, but is not identical with either of these. To this acid the author gives the name of methoxacetic acid; it is monobasic, and gives beautifully crystallized salts. The acid itself is easily prepared by decomposing the zinc salt with sulphu-

retted hydrogen, and distilling the liquid after separating the sulphid of zinc. The boiling point rises gradually till it becomes constant at 198° C., when the acid passes over as a colorless liquid, with a sour smell, resembling that of acetic acid.

When monochloracetate of soda is boiled continuously, an acid liquid passes over, which, when saturated with baryta, yields on evaporation a crystallizable salt having the formula of glycolate of baryta. The author proposes for the acid contained in this salt the name of oxacetic acid; it is isomeric, but not identical with the glycolic acid. Its formula is $C_4H_4O_6$.

Ethylate of soda acts violently on monochloracetic acid; the products of the reaction are chlorid of sodium, and the soda salt of a new organic acid, homologous with the two last described, and which the author terms ethoxacetic acid. This acid is volatile without decomposition and appears to boil at a lower temperature than the corresponding methyl compound. Its formula is $C_8H_8O_6$.

Amylate of soda acts in a similar manner on monochloracetic acid. The new acid formed in the reaction is an oily liquid which has the formula $C_{14}H_{14}O_6$.

Phenylate of soda, under similar circumstances, yields phenoxacetic acid, as an oily liquid which crystallizes at a low temperature, and distils over without decomposition. The analyses appeared, however, to show that the phenyl alcohol employed contained benzalcohol, and that the product examined was therefore a mixture of the homologous acids $C_{16}H_8O_6$ and $C_{18}H_{10}O_6$.

When the soda salts of organic acids are heated with monochloracetic acid, chlorid of sodium is formed, and new organic bodies which the author proposes to study. It is easy to see that all the acids belonging to the formic series will yield similar new acids with the peroxyds of the alcohol radicals homologous with hydrogen, and that in this manner a very great number of new compounds may be obtained.—*Journal für prakt. Chemie*, 78, *p.* 174.

Note.—According to Kolbe's view, lactic acid is to be regarded as propionic acid, in which one equivalent of hydrogen is replaced by one of HO_2, so that its rational formula is $\left.\begin{matrix} C_6H_4(HO_2)O_2 \\ H \end{matrix}\right\} O_2$. Upon this view the oxacetic acid of Heintz must be identical with glycolic acid, which appears not to be the case. The question may doubtless be decided directly by examining the products of the action of water upon chloropropionic acid, which ought thus to yield lactic acid directly, since we should have the reaction

$$C_6H_4ClO_5HO + HO_2H = C_6H_4(HO_2)O_5HO + HCl.$$

Heintz's acids may be more simply regarded as derived from the formic series by simple replacement of hydrogen by the peroxyds HO_2, $C_2H_3O_2$, $C_4H_5O_2$, &c. Dichloracetic and trichloracetic acids ought to yield analogous products in which two or three equivalents of hydrogen are replaced by two or three equivalents of peroxyds which need not be of the same radical. The number of possible acids would thus almost or quite equal that of the ammonias or ammoniums. W. G.

2. *On the chemical constitution of Isethionic acid and Taurin.*—By the distillation of isethionate of potash with perchlorid of phosphorus, Kolbe has obtained a new acid which has the empirical formula $C_4H_5ClS_2O_5$, and which he terms chlorethyl-sulphuric acid. This acid yields taurin by the substitution of NH_2 for Cl, and ethyl-sulphuric acid $C_4H_6S_2O_6$ when the chlorine is simply replaced by hydrogen. Kolbe draws a parallel between the derivatives of carbonic and sulphuric acids, which is best illustrated by the following tabular view:

$\underbrace{C_2O_2}\,.\,O_2$	$\underbrace{S_2O_4}\,.\,O_2$
Carbonic acid.	Sulphuric acid.
$HO\,.\,\underbrace{(C_4H_5)}(\underbrace{C_2O_2})\,.\,O$	$HO\,.\,\underbrace{(C_4H_5)}(\underbrace{S_2O_4}),O$
Propionic acid.	Ethyl-sulphuric acid.
$HO\,.\,\underbrace{(C_4H_4Cl)}(\underbrace{C_2O_2}),O$	$HO\,.\,\underbrace{(C_4H_4Cl)}(\underbrace{S_2O_4}),O$
Chloropropionic acid.	Chlorethyl-sulphuric acid.
$\underbrace{(C_4H_4Cl)}(\underbrace{C_2O_2}),Cl$	$\underbrace{(C_4H_4Cl)}(\underbrace{S_2O_4}),Cl$
Chlorethyl-carbon-chlorid.	Chlorethyl-sulfochlorid.
$HO\,.\,\underbrace{(C_4H_4NH_2)}(\underbrace{C_2O_2}),O$	$HO\,.\,\underbrace{C_4H_4NH_2)}(\underbrace{S_2O_4}),O$
Amidoethyl-carbonic acid.	Amido-ethyl-sulphuric acid.
(Alanin)	(Taurin).
$HO\,.\,\underbrace{(C_4H_4(HO_2))}(\underbrace{C_2O_2}),O$	$HO\,.\,\underbrace{(C_4H_4(HO_2))}\underbrace{S_2O_4},O$
Oxethyl-carbonic acid	Oxethyl-sulphuric acid.
Lactic acid.	Isethionic acid.

The author promises a more detailed account of the compounds and reactions referred to in the above brief preliminary notice.—*Ann. der Chemie und Pharm.*, cxii, 241.

3. *Researches on the atomic weight of Graphite.*—Brodie has communicated an exceedingly interesting and suggestive memoir on the atomic weight of graphite, considered as an allotropic form of carbon, the fundamental idea being that the different modifications of the same substance may exhibit a difference in equivalents, as well as in their ordinary chemical and physical properties. The author finds that graphite, when heated with nitric acid and chlorate of potash, increases in weight, and ultimately yields a light yellow crystalline substance. The details of the process are as follows: a portion of graphite is intimately mixed with three times its weight of chlorate of potash, and the mixture placed in a retort. A sufficient quantity of the strongest fuming nitric acid is added to render the whole fluid. The retort is placed in a water-bath, and kept for three or four days at a temperature of 60° C. until yellow vapors cease to be evolved. The substance is then thrown into a large quantity of water and washed by decantation nearly free from acid and salts. It is then dried in a water-bath, and the oxydizing operation repeated with the same proportion of nitric acid and chlorate of potash, until no farther change is observed. This is usually after the fourth time of oxydation. The substance is then to be dried, first in vacuo, and then at 100°. By placing the mixture in a flask exposed to sunlight, the change takes place more rapidly and without the application of heat.

The formula of the body thus obtained is $C_{22}H_4O_{10}$, or, as the author writes it, $C_{11}H_4O_5$.

Its crystals belong either to the right or oblique prismatic system. It is insoluble in water, containing acids or salts, and very slightly soluble in pure water. It unites with alkalies, and the crystals have an acid reaction: ammonia converts it into a transparent jelly, but the substance is not dissolved. Acids separate it from this combination, as a gelatinous mass resembling silica. Treated with deoxydizing agents, it is readily decomposed. When a solution of sulphate of ammonium or of potassium is poured upon the dry substance, a crackling sound is heard, and a body is formed resembling graphite.

The crystals are decomposed with ignition on the application of heat, gases being evolved, and a black residue left, which resembles finely divided carbon. This substance the author proposes to term graphic acid.

When graphic acid is heated in naphtha to about 270°, water and carbonic acid are given off, while the naphtha takes a deep red color. The residual substance resembles graphite and has the formula $C_{44}H_2O_8$, or with the author's equivalents, $C_{22}H_2O_4$. When this substance is heated in a current of nitrogen to a temperature of 250°, water is given off with a little carbonic acid; the substance remaining is found to have the formula $C_{132}H_4O_{22}$ or $C_{66}H_4O_{11}$. This body may be exposed for several hours to a red heat in a current of nitrogen without losing all its oxygen and hydrogen.

The author compares graphic acid with a remarkable compound of silicon discovered by Buff and Wöhler, which has the formula $Si_4H_4O_5$, and which was obtained from the graphitoid form of that element. The properties of the two substances agree very closely, whence it may be inferred that the graphite compound is the same term in the system of carbon as the silicon compound in the system of silicon. The total weight of graphite which in the compound is combined with atoms of hydrogen and of oxygen is 132. If we assume that this weight is like the corresponding weight, 84· of silicon, to be divided into four parts, we arrive at the number 33 as the atomic weight of graphite. Representing this weight by the letters Gr, the formulas of the substances $C_{11}H_4O_5$, $C_{22}H_2O_4$, and $C_{66}H_4O_{11}$ become $Gr_4H_4O_5$, $Gr_8H_2O_4$ and $Gr_{24}H_4O_{11}$, where O=16.

According to the law of Dulong and Petit, the specific heats of the elements are inversely as their equivalents. The elements are divided into two classes, one in which the product of the specific heat into the equivalent is about 3·3—the other in which this product is 6·6. The specific heat of carbon in the form of graphite—0·20187—presents a remarkable exception to the law, if we take its equivalent as 6· or 12, but if we assume the atomic weight of graphite as 33, we have for the product of the specific heat into the atomic weight, the number 6·6 which is according to the law of Dulong and Petit.

The relation which exists between the atomic weights of boron, silicon and zircon, and that form of carbon for which a place may be claimed as a distinct element, *graphon*, is precisely the kind of numerical relation which is found to exist between the weights of analogous elements. We have

Boron	11
Silicon	21
Graphon	33
Zircon	66

These considerations lead to the inference that graphite functions as a distinct element, forming distinct combinations with a distinct equivalent, viz: 33. How far this inference may be extended to the allotropic forms of other elements, experiment alone can decide.—*Quart. Journal of Chem. Soc.*, vol. xii, p. 261.

[*Note.*—With respect to the numerical relations between the eqs. of boron, silicon, graphon and zirconium which Brodie points out, it may be remarked that boron—at least with the equivalent 11—is triatomic, as shown by the density of the vapor of BCl_3 and other considerations. It cannot, therefore, with this equivalent, belong to the same natural group with silicon and zircon, which are diatomic, as shown by recent investigations. Marignac has established the isomorphism of SnF_2+KF with SiF_2+RF, while Troost and Deville have shown from the vapor-density of chlorid of zirconium that its true formula is $ZCl_2=2$ vols. or Z_2Cl_4 if we assume that all compounds correspond to 4 vols. in a gaseous state. The vapor density of $SiCl_2$ also agrees with the supposition that silicon is diatomic, supposing it to represent 2 vols. The true equivalents of silicon and zirconium become therefore respectively 14 and 44 or 28 and 88, if we admit the 4-volume theory. The equivalents of carbon, silicon and zirconium are then to each other as 6, 14 and 44, or as 12, 28 and 88, the common difference being 8 or 16 nearly. The formula $Si_4H_4O_{10}$ was deduced by Buff and Wöhler upon the supposition that the equivalent of silicon is 21, the element being triatomic as assumed by Berzelius. But if we take 14 as the true equivalent, the formula for the same compound becomes $Si_6H_4O_{10}$, and comparing with this the formula $C_{22}H_4O_{10}$ we have 132 parts by weight of carbon, representing 6 eqs. of graphon instead of 4, as assumed by Brodie. This gives 22 as the equivalent of graphon, instead of 33. If now we multiply the spec. heat of graphite as found by Regnault, namely 0·201, by 22 we have 4·4 so that the spec. heat of an atom of graphon does not obey the laws of Dulong and Petit, as the product should be either 3·3 or 6·6. It may, however, be remarked that the spec. heat of graphitoid silicon has not yet been determined, and that there may be other classes of elements whose atoms have the intermediate spec. heats 4·4 and 5·5. The formulas of Brodie's compounds become, if we take the equivalent of graphon as 22—$Gr_6H_4O_{10}$, $Gr_{12}H_2O_8$, $Gr_{18}H_4O_{22}$, (taking $O=8$ and not with Brodie as 16). No probable relation can be pointed out between the numerical values of the equivalents of graphon and of other elements, until we know to what natural group graphon belongs, since it is not certain or even very probable that the allotropic modifications of the same element belong to the same group.—W. G.] W. G.

4. *On the Cause of Color and the Theory of Light;* by Mr. JOHN SMITH, M.A. (Read by his brother, Dr. R. A. Smith).—The author, in attempting to explain certain natural phenomena, could not satisfy himself by applying the principles of either theory of light, and said that many natural phenomena indicated beats or vibrations in the luminous ether very

different from what science taught. That is, that there were greater intervals between them than Newton had demonstrated and scientific men believed. He therefore endeavored to contrive experiments by which he would be able to make as many revolutions or beats in a second as he considered the effective vibrations of light were repeated in a second of time, and argued that by certain contrivances to produce light and shade in alternate vibrations he should produce color. A series of experiments was subsequently undertaken, which led to the conclusion that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color. That is, supposing white light to consist of the motion of an ether, blackness to consist of an entire absence of motion, then a certain color, blue, red, or yellow, will be produced by the alternate action of the light and the shadow. The author used shadow in the positive sense as the sensation was positive.

On pursuing the inquiry, he first caused a small parallelogram cut in card board to revolve over a black surface with a rapidity which he considered equal to the vibration of light. By this motion he obtained a distinct blue, while at another time in different weather he obtained a purple. He then made a disc with several concentric rings, which he painted respectively $\frac{1}{3}$, $\frac{2}{3}$, $\frac{3}{4}$, and $\frac{1}{2}$ black, leaving the remainder white, and on making this disc revolve the rings became completely colored. There was no appearance of any black or white. In a bright day with white clouds in the sky, the rings were colored respectively a light yellowish green, two different shades of purple, and a pink. By using discs of a great variety of shapes and different proportions of white and black, the author said that he produced successively or together all the colors of the rainbow, although he had not yet arrived at the exact arithmetical determination of the amount of light and shade needful for each color.

These experiments were made before the Society by the light of a paraffin oil lamp with a reflector. The author said that they were much more brilliant by sunlight.

There was another set of experiments which the author considered as very effective, and especially as being easily made and described, but requiring strong sunshine to show them. These were made by casting a shadow of a particular figure on a white wall or on a sheet of paper, so as to produce alternate beats of light and shadow when put in revolution. The figure became colored of different shades, and because these could be seen on the wall, like the spectrum from the prism, he called them spectra by reflection.

He mentioned also that the colors may be produced by making a black disc, with figures cut out of it, revolve before a white cloud or white screen.

There were many others which he had no time to enumerate, much less to describe, but he described some of the figures which produce the phenomena which are perceived when looking through transparent solids.

The author considered that his theory gave an entirely new and simple explanation of the phenomena of refraction through the prism, and summed up as follows :—

The experiments prove the homogeneity of the ether.

They prove the undulatory hypothesis, but oppose the undulatory theory.

They enable us to dispense with the different refrangibilities of the rays of light, as taught by Newton.

They help to explain many of the phenomena of what is called the polarization of light.

They give a new explanation of prismatic refraction, and explain in a plain and simple manner many very interesting natural phenomena.

Startling, he said, as these conclusions are to those who are conversant with the subject of light, he thought he was perfectly warranted in drawing them from his experiments. The general process of reasoning could not, however, be given in a short abstract.—*Ordinary Meeting, Oct. 4th, 1859, Manchester Literary and Phil. Society.*

TECHNICAL CHEMISTRY.

1. *Vegetable Parchment.—Papyrine.*—The interesting substance obtained in 1846 by Poumarède and L. Figuier (Comptes Rendus, xxiii, 918; see also this Journal xxviii, 431,) by immersing bibulous paper in partially diluted sulphuric acid—called *papyrine** by its discoverers—which with the exception of a few comparatively unimportant applications in France, where it was used for the shelves on which silk-worms are reared, &c., had excited scarcely any interest other than that naturally attaching to it as a chemical curiosity, until patented (Dec. 6, 1853) in England, by Gaine, (see *Rep. of Pat. Inv.* [E. S.] xxiv, 151) and manufactured by the well known house of De LaRue & Co., of London, has recently been investigated by Prof. A. W. Hofmann, (Ann. Ch. u. Pharm., Nov. 1859, cxii, 243; from a report to Messrs. Thos. De LaRue & Co.) In its prominent properties it resembles ordinary parchment very closely: indeed the two can hardly be distinguished from each other except on close inspection. Both exhibit the same peculiar pale, yellowish tint, the same degree of translucency, the same half fibrous, horn-like texture. Like animal parchment, the artificial product is not easily torn: it may be repeatedly bent or folded without exhibiting any special appearance of breaking in the creases formed. Like ordinary parchment it is extremely hygroscopic, and becomes more pliable by absorbing moisture. When wet with water it comports itself like untanned skins, swelling up to a slippery mass through which water cannot pass except by endosmose: the coherence of the substance is not at all impaired by thus soaking.

Vegetable parchment is best prepared by immersing unsized paper during a few seconds in oil-of-vitriol which has been diluted with half its volume of water, and immediately afterwards washing it in a dilute solution of ammonia; a thorough washing with pure water completing the process. Hofmann has ascertained by direct experiment that not less than one-fourth volume, or more than one-half volume, of water must be used with one volume of monohydrated sulphuric acid, in preparing the acid bath. The paper must not be immersed too long, nor should the temperature of the bath be higher than about 15° (C.)=[59° F.] A considerable amount of practice is moreover requisite before one can obtain a perfectly satisfactory product. When paper is transformed into vegetable parchment it undergoes no appreciable increase in weight. The action

* Should not this term, which has an undoubted right of priority, be preserved as the scientific name of the substance?—[F. H. S.]

of the sulphuric acid is purely molecular, the ultimate chemical composition of the paper—*cellulose*—remaining unchanged. [As already stated by Poumarède and Figuier *loc. cit.* and by J. Barlow, Proc. of the Royal Inst. 1857, ii. 411]. The result of the momentary action of sulphuric acid in this instance is comparable with that which a longer action of this acid upon woody fibre produces, viz.: formation of dextrine, a substance well known to be isomeric with cellulose. Indeed, the vegetable parchment may be regarded as a middle term between dextrine and cellulose.

The samples of parchment-paper examined by Hofmann [and by Barlow] contained no trace of free sulphuric acid; small portions of sulphate of lime and of sulphate of ammonia being the only soluble impurities present.

There is no apparent reason why the parchment-paper should not endure for an indefinite length of time. It is evident that if its destruction were dependent in any way upon the chemicals used in preparing it, decomposition would set in at once. Nothing of the kind occurs, however. Specimens of the factitious parchment which have been in Hofmann's possession during four years being undistinguishable from those recently prepared.

From experiments made in order to ascertain the strength of parchment-paper, as compared with that of true parchment and of unsized paper, it appeared that while strips of unsized paper broke when subjected to a weight of 15 or 16 pounds, similar strips of vegetable parchment supported 74 lbs., and those of ordinary parchment 75 lbs., before breaking. The cohesive force of unsized paper is thus increased five-fold by the treatment with sulphuric acid. It was also proved by experiment that for equal *weights* of the two substances parchment-paper exhibited about three-fourths the cohesive power of animal parchment. It also appeared that while the strength of strips of parchment-paper taken from different sheets was nearly constant, that of strips of animal parchment, even when cut from a single piece, was extremely variable, owing to the differences in thickness to which it is liable.

Parchment-paper although not quite so strong as ordinary parchment, is nevertheless more capable than the latter of withstanding the action of chemical agents, and especially of resisting the action of water; it may be left in this liquid for days, or even boiled in it, without undergoing any change, other than the increase of volume already alluded to, its original cohesion, and indeed all its properties being regained on drying. As is well known, animal parchment is soon converted into glue when boiled with water.

Since the parchment-paper contains no nitrogen, it is much less liable than ordinary parchment to putrefy when exposed to moisture, and will probably be less subject to the attacks of insects. Not only may the new parchment be substituted for that ordinarily employed for legal documents, &c.; but from its cheapness it will probably soon be used for ledgers and other important records—possibly for bank-notes—instead of the more perishable paper now employed. ["It will take the place of ordinary paper in school books, and other books exposed to constant wear." "It also promises to be of value for photographic purposes, and for artistic uses, in consequence of the manner in which it bears both oil and water-

color."—*Barlow.*] Its strength and power of resisting the action of moisture seem also specially to adapt it for the use of architects and engineers —particularly for working-plans liable to receive rough usage; also for the envelopes of letters and for cartridges. In thin leaves it affords an admirable tracing paper. As a material for binding books it will without doubt be extensively used. The ease with which it receives both printers' and ordinary writing ink is remarkable. For chemical laboratories it affords a most convenient material for fitting together retorts, condensers, and the like; while its power of resisting the fluids used in galvanic batteries suggests that it may be useful for diaphragms, &c. It is already used by tons, instead of bladder, as a covering for jars containing preserves, marmelades, etc.

Parchment-paper has been successfully manufactured on the great scale for a year or more by the firm of De LaRue, the numerous difficulties which presented themselves having been fully overcome by the perseverance of one of its members—the distinguished chemist Warren De La-Rue. [Specimen sheets of the parchment-paper accompany Hofmann's memoir.—R. H. S.].

2. *Weighing of Moist Precipitates;* by FERDINAND F. MAYER.—Mr. Ch. Mène, of Creusot,* gives a mode of weighing which does away to a great extent with the tediousness and difficulties attending the drying of many precipitates, especially in volumetric analysis. He washes the precipitate thoroughly by decantation, and then introduces it carefully into a bottle, the exact weight of which, when filled with distilled water at a certain temperature, is known. Since the precipitate is heavier than water, the bottle when filled again will weigh more than without the precipitate, and the difference between the two weights furnishes the means of calculating the weight of the precipitate.

In case the precipitate settles but slowly it may be collected on a filter, and together with the filter, after washing, be introduced into the bottle, in which case the weight of the filter and its specific gravity, supposing any difference should exist between its own and that of water, is to be taken in account. Precipitates soluble in or affected by water may be weighed in some other liquid.

This method, of which the above are the outlines, is spoken of in the Jahresbericht der Chemie for 1858† in rather disparaging terms, and I consider it not more than justice to the method, if not also to Mr. Mène, to prove its correctness, the more so as I have applied the principle on a large scale as far back as 1855.

I engaged in that year in the manufacture of carbonate of lead from refuse sulphate of lead, by treating the latter in a pulpy condition with carbonate of soda. The sulphate of lead I used contained very varying proportions of water and soluble impurities, from which latter it had first to be freed by washing. It was then in the state of a thin pulp, and the difficulty was to find the amount of dry sulphate of lead, as it was a matter of importance to use as little carbonate of soda, and to obtain as pure a carbonate of lead and sulphate of soda as possible. This could only be done by weighing it as a whole, or in portions; but as the

* Journal de Pharmacie et de Chemie, Oct. 1858.

† Jahresbericht der Chemie, by Will and Kopp, for 1858, p. 6.

drying of a tubful of sulphate of lead (from 500 to 1200 pounds) was impracticable, and sampling not less so, since the upper strata contained a much larger proportion of water than the lead at the bottom: I contrived the following method, which enabled me to leave the management of the process in the hands of a workman.

I took a strong oaken pail, weighing eight pounds when empty, and caused a black mark to be burnt in horizontally around the inside of the pail, two inches below the rim, up to which mark it held twenty pounds of water. I reasoned as follows: The specific gravity of sulphate of lead being 6·3, the pail if filled up to the mark would hold 126 pounds of pure sulphate of lead. The specific gravity of water being 5·3 less than that of sulphate of lead, it followed that if there was one pound of water in the pailfull of moist sulphate, the pail would weigh 5·3 pounds less than 126 (+8, the tare of the pail) =120·7 (+8); if there were two pounds of water present, the weight would be 115·4 (+8), and so on. This enabled me to calculate a table, giving in one column the actual weight of the pail when filled with moist sulphate, and opposite in a second column, the amount of dry sulphate corresponding to the gross weight. The weight of dry sulphate was thus found as accurately as could be desired, although the amounts varied in practice from 30 to 105 pounds.

It is nothing but an application of the Archimedean theorem, that, when a solid body is immersed in a liquid it loses a portion of its weight, equal to the weight of the fluid which it displaces, or to the weight of its own bulk of the liquid.

This, as I suppose, is precisely the principle applied by Mr. Mène. *The precipitate he obtains by a certain chemical manipulation is a substance of known composition and specific gravity.* Supposing it to be sulphate of lead, and the bottle, when filled with water at the normal temperature, to weigh 70 grammes =50 grammes of water, and 20 for tare. After introducing the precipitate and filling again with water it weighed 71·06 grammes. Now, as the specific gravity of sulphate of lead is 6·3, or as the weight of a cubic measure of sulphate of lead is 6·3 times that of a cubic measure of water, and as the space of one part by weight of water is taken up by 6·3 parts by weight of sulphate of lead, it follows that the quantity of sulphate of lead in the bottle, which has taken up the space of one part by weight of water, increases the original weight of the bottle (filled with pure water) by 5·3. To find the amount of water displaced it is only necessary to divide the overweight (1·06 grammes) by 5·3=0·2, which, added to the overweight 1·06+0·2 gives 1·26 grammes as the weight of the precipitate.

Hence the rule, which is of great convenience in volumetric analysis, that *to find the weight of a moist precipitate, which is a compound of known specific gravity, weigh it in a specific gravity bottle or some other vessel of known weight when filled with water, or any other liquid, at the normal temperature, again fill it with the water or other liquid, divide the excess of the new weight by the specific gravity of the substance, less that of the water or other liquid* (*that of water being* =1) *and add the quotient to the overweight, which gives the weight of the precipitate.*

The editor of the Jahresbericht appears to have overlooked the fact that the precipitates weighed in this manner are definite compounds, the specific gravity of which is well ascertained.

The principle I have exemplified above may not be novel; but as I have never met with it, chemists, as well as manufacturers (especially of colors), will probably also find it of interest, and certainly highly practicable and easy of execution.

36 Beekman street, New York, Feb. 3d, 1860.

3. *New Chemical Journal.*—The Chemical News (with which is incorporated the Chemical Gazette), edited by WILLIAM CROOKES. London: Weekly. Price 3*d*., stamped 4*d*. 8vo. 12 p. each number. This new Journal commenced on the 10th of December last, and eight numbers have already reached us. The contents are divided under Scientific and Analytical Chemistry, Technical Chemistry, Pharmacy, Toxicology, &c., Proceedings of Societies, Notices of Patents, Correspondence, Scientific Notes and Queries, Laboratory Memoranda, Miscellanies, and Answers to Correspondents. Mr. Crookes is favorably known by several valuable researches, and thus far has shown good judgment and spirit as an editor. His verbatim reports of the late lectures of Dr. Faraday (the Holiday lectures) at the Royal Institution, attest his appreciation of the true sources of vitality for such a journal.

4. *American Druggists' Circular and Chemical Gazette;* N. Y. Feb. 1860. 4to.—Although chiefly special and wholly technical in its objects, this Journal (which has now reached its 4th volume, whole number 38) is conducted by Mr. Mayer and others, in a manner to entitle it to rank as a valuable coadjutor in technical chemistry.

II. GEOLOGY.

1. *On some of the Igneous Rocks of Canada;* by T. STERRY HUNT, F.R.S. (In a letter to one of the editors, dated Jan. 1860.)—There occurs in the district of Montreal a series of isolated hills running nearly east and west for a distance of ninety miles along the line of an undulation which has disturbed the lower Silurian strata. These hills, which often cover considerable areas, consist of igneous rocks which have apparently been solidified under a considerable pressure, and have subsequently been exposed by the denuding action which has removed from around them the soft and unaltered palæozoic strata. The names of these mountains counting from the west are Rigaud, Mount Royal, Montarville, Belœil, Rougemont, Yamaska, Shefford and Brome, to which we may add Monnoir a similar mass lying somewhat to the south of Belœil.

I am now engaged in the study of the various rocks composing these mountains, which offer great diversities in lithological character and composition. Prominent among them we may mention the trachytes, which in their various types of compact, granular, porphyritic and granitoid are abundant. The mountains of Brome and Shefford appear to be made up entirely of a granitoid trachyte, which consists of crystalline orthoclase, without quartz, and with small portions of hornblende or mica, sphene and magnetite. The orthoclase in a great number of these rocks which I have analyzed contains like sanidin a large proportion of soda. The other varieties of trachyte which occur in veins and dykes often contain a portion of carbonates amounting to from 6·0 to 18·0 p. c. and consisting chiefly of carbonate of lime with some magnesia and

iron. Some of these rocks pass into phonolites through the admixture of a silicate which gelatinizes with acids and has the composition of natrolite. This mineral in one case amounted to more than 40·0 p. c. of the rock, the remainder being orthoclase with a small amount of carbonates.

A large part of the mountain of Yamaska consists of a coarsely crystalline diorite the feldspar of which approaches anorthite in composition, while an apparently similar diorite, which makes up the mass of Monnoir, contains oligoclase in large crystals. Other diorites from this series contain labradorite; mica and sphene, in small quantities, are often present.

Dolerites are also abundant, and sometimes pass, owing to a scarcity of feldspar, into an augite rock, generally with ilmenite and magnetite. A fine-grained dolerite from Rougemont contains abundance of crystallized olivine, and a large part of Montarvilles consists of a remarkable granitoid rock, made up of a crystalline feldspar, in some parts at least labradorite, with sparsely disseminated crystals of black augite, a little brown mica, and a great abundance of crystals of honey-yellow olivine, which amount to more than 45·0 p. c. of the mass. The composition of this olivine I have found to be silica 37·17, magnesia 39·68, protoxyd of iron 22·54 = 99·39.

Many of these diorites and dolerites, except in their lithological structure, closely resemble the stratified rocks made up of anorthic feldspar, with hornblende and pyroxene, and containing magnetite and ilmenite, which are so abundant in the Laurentian system, suggesting the notion that the intrusive masses may be nothing more than these stratified rocks displaced and injected among the Palæozoic strata. Durocher has already pointed out a similar resemblance between the intrusive rocks of some parts of Scandinavia, and the subjacent gneiss.—(*Bul. Soc. Geol. France*, [2] vi, 33.)

The granitoid trachytes as well as the dolerites, diorites and peridotite (olivenite rock) make up mountain masses, while the earthy and porphyritic trachytes and the phonolites are generally found cutting the above, and the adjacent strata. The absence of quartz or of any excess of silica from all these rocks is a remarkable feature. Farther to the east, however, intrusive granites are very abundant; these penetrate the Devonian strata but are older than the carboniferous. Quartziferous plutonic rocks are also abundant in the county of Grenville, where they penetrate the Laurentian series. These plutonic rocks consist of dolerites, syenites and eurites, which are in their turn cut by dykes of very beautiful porphyries. The base of these is jasper-like, black, red or green in color, and encloses crystals of red orthoclase and occasional grains of quartz. The analysis of the base shows it to consist of the elements of orthoclase with an excess of silica and a little oxyd of iron. The syenites are cut by large veins of chert, and in the vicinity of these have been changed into a sort of kaolin from a decomposition of the feldspar, which may have been the source of the silicious accumulations. This group of igneous rocks, which is overlaid by the Potsdam sandstone is very unlike those which we find penetrating the palæozoic strata.

The details of my investigations on these rocks, so far as completed, will be found in the Report of the Geological survey of Canada, for the last year, now in press; a first portion has already appeared in the Report for 1853–56, p. 485. It is by the systematic study of different series of igneous rocks that we may hope to arrive at just notions as to their origin, their mode of formation and their relations to metamorphic sedimentary rocks.

2. *Notes on the Dolomites of the Paris Basin, etc.;* by T. Sterry Hunt, F.R.S. (In a letter to one of the Editors, dated Montreal, Feb. 3, 1860.)—The gypsums of the Paris basin are evidently not of epigenic origin but regularly stratified and alternating with marls and limestones. In September, 1855, I visited with Elie de Beaumont and several members of the Geological Society of France, the gypsum quarries at the hill of Chaumont, and there insisted upon the views which I have since urged in this Journal (vol. xxviii, p. 365) upon the different origins of gypsum. For a report of my remarks on that occasion see the *Bulletin de la Société Geologique de France*, [2], xii, 1306.

I have subsequently shown in the memoir just cited in the last volume of this Journal that the formation of these stratified gypsums by the double decomposition of bicarbonate of lime and sulphate of magnesia involves the production of carbonate of magnesia, which unless carried away or decomposed by an irruption of sea water will be found to overlie the gypsums forming the dolomite which is their common associate. The presence of carbonate of magnesia in the gypsiferous series of the Paris basin has hitherto been unnoticed, and having at the time above mentioned collected specimens from the quarries at Chaumont I was recently induced to examine the so-called *white marls* which overlie the gypsiferous series, and find them to be magnesian. The analyses of two specimens, one penetrated by seams of gypsum, gave each about 60·0 per cent of dolomite mingled with clay. The Paris gypsums then offer no exception to the general rule.

Beneath the gypsiferous series and in the lacustrine group known as the lower travertine, or St. Owen limestone, occur beds of a whitish, very fissile, shaly matter, enclosing concretions of menilite (opal), and consisting of a hydrated silicate of magnesia, identical with meerschaum or quincite in composition, intermingled with small portions of earthy carbonates. I have examined a specimen of this mineral which I collected near Paris and find it to be the same with that described by Dufrénoy and Berthier as occurring in similar positions in various other localities. This appearance of beds of a silicate of magnesia approaching talc in composition, in the midst of unaltered deposits, is interesting inasmuch as it seems to show that such silicates may be formed in basins at the earth's surface by the reactions between magnesian solutions and dissolved silica. I have many years since described the existence of similar silicates among the deposits during the artificial evaporations of natural alkaline waters, and farther inquiries in this direction may show us to what extent certain rocks consisting of calcareous and magnesian silicates may be directly formed in the moist way.

I propose to send you very soon a supplement to my paper on Gypsums and Magnesian rocks, describing some recent experiments which

confirm what I have already announced that no dolomite is formed in the experiment of Von Morlot and Haidinger, recently resuscitated by Charles Deville, and appealed to by Prof. Phillips in his last Annual Address to the Geological Society of London, as resolving the problem of the origin of dolomite. This double salt is however readily formed when a mixture of the moist amorphous carbonates (such as is obtained by precipitating in the cold by an excess of carbonate of soda a solution of the chlorids of calcium and magnesium in equivalent proportions), is gradually heated under pressure.

3. *New Palæozic Fossils;* by J. H. McChesney. Chicago, 1859. 8vo. pp. 64. In this publication the following species from the Carboniferous rocks of the Western States are noticed as newly described:

Crinoidea.—*Platycrinus ornogranulus, P. inornatus, Scaphiocrinus longidactylus, Zeacrinus bifurcatus, Z. mucrospinus, Actinocrinus asterius, A. tenuisculptus, A. subæqualis, A. Fosteri, A. subventricosus, A. urnæformis, A. Hurdianus, A. æquibrachiatus, A. Andrewsianus, A. Hageri, Forbesiocrinus Pratteni.*

Brachiopoda.—*Orthis, Kaskaskiensis, O. Lasallensis, O. Pratteni, O. Richmonda, Productus asperus, P. symmetricus, P. Wilberanus, P. tubulospinus, P. fasciculatus, P. inflatus, P. pileiformis, Amboccelia gemmula, Spirifer transversa, S. subelliptica, S. perplexea, S. subventricosa, Retzia subglobosa, Athyris spiriferoides, A. orbiculuris, A. differentius, Terebratula inornata, Rhynchonella Eatoniæformis, R. explanata, R. carbonaria, R. Algeri, Trematospira Mathewsoni, Discina caputiformia.*

Lamellibranchiata.—*Leda Oweni, L. gibbosa, L. polita, Nucula parva, N. cylindricus, N. rectangula, Asturtella varica, Edmondia concentrica, Allorisma clavata, A. sinuata, Myalina Swallovi, Nuculites Vaseyana, Pinna Adamsi, Syringopora multattenuata, Cyathoxona prolifera.*

Gasteropoda.—*Bellerophon ellipticus, B. vittatus, B. Blanyana, B. Stevensiana, Pleurotomaria Beckwithana, P. nodomarginata, Natica Shumardi, Platyceras crytolites, Platyostoma Peoriensis, Bucania Chicagoensis.*

Cephalopoda.—*Nautilus Forbesianus, N. Illinoiensis, N. quadrangulus, N. nodocarinatus, Gonitites Hathawana, Cyrtoceras (Lituites?) giganteum, Trochoceras Desplaniensis, Orthoceras Rushensis, O. Knoxensis.*

The typography of this brochure is very good, and the descriptions of the species show that the author has a wide acquaintance with the Carboniferous fauna of the West. We think, however, that the science would present to the student a much less formidable array of difficulties, if Palæontologists would on all occasions give accurate measurements of the species they describe. Throughout this book, for instance, the size of the individual is recognized as a specific character, and yet as no dimensions are given it must be impossible for even the most experienced practical naturalist to decide whether *O. Lasallensis, O. Richmonda* and *O. Pratteni* are small or large forms; whether two lines or two inches wide. The first mentioned of these species is also said to have the "*surface marked by sharp rugose radiating striæ increased by implantation,*" but so have a great many other *Orthides*, and until some standard for comparison is furnished, all the students of this book, except those who may have access to the original specimens, must remain in doubt upon the question as to whether there are ten or twenty striæ in the width

of one line. We find it stated further that in *O. Richmonda* the striæ are "finer and less rugose" than they are in *O. Lasallensis* while in *O. Prattenі* they "are not so distant and not so rough," as in either of the other two, but as we have no means of ascertaining their size in the first named species, which is here made the standard, all that relates to the surface characters of the second and third might have been left out without detracting anything from the value of the description. No doubt all this appeared to be sufficiently clear to the author while he was engaged with the specimens before him, but we think upon a little reflection he will agree with us, that without some clue to the size and surface characters of these species, they cannot be identified. Where a shell is said to be smaller or larger than some other species which has been described in some other book, the difficulty would be somewhat less, provided that access could be had to that book, and even then, such questions as "how much smaller?" or "how much larger?" cannot be decided. By adding two or three lines to their descriptions, palæontologists may add a vast deal to the value of their labors, and save others engaged in the same pursuits much perplexing, and too often fruitless, intellectual toil. It is not always possible to give figures of new species, but it is easy to furnish measurements. The absence of these is the great defect in the work before us, and it is a defect that may be observed in books of much greater pretensions. We notice a new genus, (AMBOCELIA) among the Brachiopoda, which is said to have been "recently established by Prof. James Hall," in the "Regents' Report of the State of New York for the present year," (meaning 1859). Has this report been published? If not, then the genus is not established. No author can establish a genus before he has published his description, and, even then, he may not succeed in shewing that it is new.

The practice of antedating genera and species should be discountenanced, as it cannot be beneficial to science. Where specific names are derived from names of persons or places, the initial should be a capital letter, a rule which has been of late much disregarded.

4. *Explorations in Nebraska.*—Dr. F. V. HAYDEN, in a letter to Prof. DANA, dated Deer Creek, Nebraska, Dec. 1, 1859, states, after speaking of the interest of the region, that as soon as the grass is sufficiently high this coming Spring, and the swollen streams permit the passage of pack trains (which cannot be sooner than the first of June), Capt. Reynolds proposes to divide his party into two divisions—one party in charge of Lieutenant Maynadier, Asst., will pass up the Wind River Valley along the western side of the Big Horn Mountains. The other division under Capt. Reynolds will proceed up the Platte to the South Pass, exploring the Wind River Mountains—the two parties to spend the 4th of July at Clark's Pass. Here Lt. M.'s party will proceed down the Yellowstone to Fort Union, and Capt. R.'s party will reach the same point by crossing at the head waters of the Missouri and descending that stream, and thence to the States as soon as possible. Such is the programme of explorations; it may be slightly varied, but in any case will traverse a country very interesting for geology.

Dr. Hayden, in addition to his geological labors, has since 1852 interested himself in ethnographical studies, and will have by the end of this trip material enough for a large volume on this subject.

5. *Geological Surveys of South Carolina and Kentucky.--The geological survey of South Carolina* has unfortunately been stopped by the unwise action of the legislature of that State at its last session. Dr. O. M. Lieber, late the State Geologist, is occupied in preparing his final report, which will embrace Anderson and Abbeville district, and a part of Edgerfield. Dorn's gold mine is described in it.

The geological survey of Kentucky is continued with unabated zeal.* We have received the Synoptical Report for the past year—a brochure of 50 pages—by Dr. D. D. Owen, principal, aided by S. S. Lyon and Jos. Lesley, Jr., Topographical Assistants, Leo Lesquereux, Palæontological Assistant, and Dr. Robert Peter, Chemist. Dr. Owen here expresses the opinion that "the report of Mr. Lesquereux of the last season's work (now completed), is by far the most practically useful geological report on this subject (coal), which has ever appeared, not only in the United States but in any part of Europe."

In his synopsis of this forthcoming report, Mr. Lesquereux says, "The third section of the report contains a short comparison of the distribution, geologically and geographically, of the coal strata in Kentucky, Ohio and Pennsylvania. This comparison is of high scientific interest, as it fixes the general distribution of the coal strata in the whole extent of the coal basins of the United States, and cannot but give to the geological reports of Kentucky a great value as containing the key of the general distribution of the coal. Henceforth all the reports treating of the distribution of coal strata will naturally take their guide and standard of comparison from the section in the Kentucky coal-fields."

6. *First report of Progress of the Geological and Agricultural Survey of Texas;* by B. F. Shumard, M.D., State Geologist. Austin, Texas, 1859. pp. 17.—We learn from this brochure that satisfactory progress has been made in the preliminary reconnaissance of the vast territory (237,500 square miles) included within the State of Texas. Dr. Shumard states that a more complete series of the geological formations exists in Texas than in any State of the Union, ranging from the Potsdam sandstone to the latest Tertiary. He has made no less than *seven* lines of section extending in various directions a collective distance of 1220 miles, determining his levels by the barometer. He has found time also for minute and final surveys of eleven counties and partial surveys of several others. The coal measures cover an area not less than four thousand or five thousand square miles, with a thickness of eight or nine feet of coal in about 300 feet of coal rocks. The coal is good in quality. Extensive beds of brown coal also occur in the Tertiary rocks in the eastern and middle portions of the State.

The fossil forms of the various strata in Texas are very abundant, and Dr. Shumard informs us that his collection is already very rich.

From the head waters of the Brazos river he has been fortunate enough to obtain a fine mass of meteoric iron weighing about 320 pounds, and a smaller mass of the same kind from Denton county, specimens of which have reached us.

* Notwithstanding the cramped appropriation of money for the two years past —two-thirds of the whole sum being consumed by expenses of publication, which is an absurdity unworthy of the spirited state of Kentucky.

7. *Post-pleiocene Fossils of South Carolina;* by FRANCIS S. HOLMES, A.M., &c. Nos. 6 to 10 inclusive, containing plates 11 to 20 inclusive. Quarto. Charleston, S. C., 1859; Russell & Ivens.—This beautiful monograph, previously noticed,* is continued in the same excellent manner in the numbers now received. The description and figures of the mollusca end with the part containing Nos. 6 and 7, and with Nos. 8, 9, and 10 commences a "Description of Vertebrate Fossils, by Prof. Jos. LEIDY." The student will refer to this memoir with particular interest at the present moment, when so much attention is being given to the occurrence of human reliquiæ with the remains of animals heretofore judged to be extinct before the human epoch. The Eocene and Post-pleiocene beds on the Ashley River are exposed to the wash of the water, and "the fossils washed from them form part of the shingle on the shore, and here become mingled with the remains of recent indigenous and domestic animals, together with objects of human art." Of those vertebrate remains actually obtained in excavations of the Post-pleiocene and Eocene formations, more confidence is felt in determining the actual age to which they belong. Both the collections submitted to Dr. Leidy by Prof. Holmes and Capt. Bowman, contain remains of the horse, ox, sheep, hog and dog, which we feel strongly persuaded, with the exception of many of those of the first mentioned genus, are of recent date, and have become intermingled with the true fossils of the Post-pleiocene and Eocene periods on the Ashley River and its tributaries. In regard to the remains of the horse from the facts related (in this memoir) we think it must be conceded that several species of this animal inhabited the country of the United States during the Post-pleiocene period, contemporaneously with the mastodon, the giant sloth, and the great broad-footed bison."—pp. 99–100.

8. *Assiniboine and Saskatchewan Exploring Expedition;* by HENRY YOULE HIND, M.A., Prof. of Chemistry and Geology in Univ. of Trinity Coll., Toronto. Toronto, 1859. 4to, pp. 202, with many maps, sections and plates.—This valuable Report comes to hand at too late an hour to enable us to do more than give its title, deferring to our next an analysis of its contents.

9. *Geology for Teachers, Classes, and Private Students;* by SANBORN TENNEY, A.M., Lecturer on Physical Geography and Natural History in the Mass. Teachers' Institutes. 12mo, pp. 311. Philadelphia, 1860.—As the scope of this little work comprises the whole range of geological phenomena, it gives necessarily a very concise account of the various departments included. The matter however seems in most cases well selected, and shows the author to be acquainted with his subject. The book is illustrated by some 200 good wood-cuts, and has one excellent feature;—that the illustrations are taken, whenever possible, from *American* objects, which cannot be said of most geological text-books previously published in this country, the authors of which have generally gone to Europe for their examples. American geology should be the main subject of American text-books, and it is pleasant to see a step taken in the right direction. Our author however is not quite up to date in some instances, as in ignoring our Permian and western Jurassic beds; in including the Lias in the Oölitic system, etc. W. S.

* This Journal, xxvii. 156.

III. ZOOLOGY.

1. *On Botanical and Zoological Nomenclature;* by Wm. Stimpson.—A more careful attention to the subject of nomenclature is urgently demanded of the followers of all branches of Natural History. It is a subject to which too little attention has been paid in an abstract or general sense, and too much perhaps in particular cases. A comprehensive code of rules, recognised by the authority of the greater lights of science, has been always needed. This was attempted during the last century by Linnæus and Illiger, and in 1842 "Rules of Nomenclature" were drawn up by the British Association, and ratified by the American Association in 1845. These are excellent as far as they go, but need much extension and many additions, as any one may observe who attempts to decide by them all questions which occur in his experience.

On the other hand, in particular cases of species and genera, the discussion of questions of nomenclature has reached such a pitch that it is no uncommon thing to see the greater part of a new zoological work devoted to synonymy. One author, after six pages of historical and synonymical matter, evincing great critical acumen and much bibliographical research, will arrive at what appears to him to be a certain and final conclusion that the true *Orthonymus aliquis* is such and such a species. The next writer who succeeds him in the same field will triumphantly prove in *ten* pages that it is not that species at all, but the *O. neminis*. And so on to the end of the chapter, if it ever will have an end, which is doubtful unless some decided action is soon taken by naturalists for the purging of their favorite science from this opprobrium. After all the pages which have been written upon some of these cases we seem no nearer to a settlement than at first. The difficulty increases rather than diminishes, —each succeeding author putting forth views differing from those of his predecessors. All this discussion, let us bear in mind, is merely preliminary, and for the purpose of indicating with certainty an object about which the author has perhaps not a dozen words to say.

Now it may appear at the first glance that the application of the law of priority is exceedingly simple. The name given by the first describer of a genus or species is to be respected, and applied to that genus or species throughout all time. But as soon as we come to apply this rule, we find cases without number in which complications occur, rendering limitations of the law necessary. Genera are to be subdivided, and are subdivided with different limits by different authors; the species of one are found by another to include two or three distinct forms, and so on. Some of the limitations of the law of priority have been laid down in the "Rules" of the British Association, but not enough to enable us to decide half the cases which may arise,—leaving the remainder subject to the whims or dependent upon the extent of the knowledge of the author who would follow them.

In applying the great law, the most difficult question of all immediately arises,—What constitutes a description? or, When has an author so designated his species that his name for it should hold? On this subject we have every variety of opinion, from that of the German ornithologists, who consider that a simple published name, referring to a specimen

in a museum, is sufficient, to that of the lamented Edward Forbes, who once insisted that no name proposed should be accepted unless accompanied by a *Latin* description or an illustrative figure. The first opinion we believe to be scouted by nine-tenths of living naturalists;—the second appears to be too stringent, as an author can of course write better in his own language than in any other, though we doubt if a description appearing in Chinese would gain the least notice from modern naturalists.

The question, "What constitutes a description," can never be decidedly answered. No rule can be proposed which is universally applicable. With regard to its *length*;—we may say that two words are not sufficient, an hundred are; but where shall we draw the line? The two sentences of one author may be better than the two pages of another. One writer will describe an object well except in one point, in which from defective observation, a character is represented in exact opposition to the true state of the case. Some descriptions are sufficient to enable the naturalists of one country, from their collateral knowledge, to determine a species, while those of another country or continent would be left entirely in the dark. An author may publish descriptions in a work for private distribution, which will be inaccessible to the great body of naturalists. We might fill many pages with such cases as these, and yet, were rules made out applicable to each, there would still be cases constantly arising which could be decided by none of them. How then can the matter be settled in these latter instances? We will suggest a method further on.

It will be observed that it is among the more common and earliest-described species that the synonymic heap is greatest. This is exceedingly embarrassing to the student, who in general has occasion to use these very species, being those most easily accessible, in the course of his studies. He may find in a dozen different books the characters, anatomical or otherwise, of what appear to him a dozen different objects, since the names used may be different, and elementary works cannot be expected to go into synonymical details. At the present day, thanks to the advance of knowledge and precision, and the international exchange of scientific works, the name of an entirely new genus or species may escape the burden to which that of older species is subjected. It is with those published in the last century that the greatest trouble occurs. Investigators among antique and forgotten books are constantly finding some obscure work or paper, perhaps scarcely known out of its immediate vicinity even at the time it was published, in which names occur which must be adopted, in the opinion of some, to the exclusion of the familiar titles which have been used for half a century. The disinterment of Klein's name *Cyclas* is an instance of this. How strange it must seem to a conchologist of the present day to be obliged to designate the common marine *Lucina* by a name which has been in use seventy years for a freshwater bivalve, while this freshwater bivalve becomes *Sphærium*; and to use *Cyclostoma* for *Delphinula*, *Terebellum* for *Turritella*, etc. The restoration by G. R. Gray of Boddaert's names in ornithology is another instance. By the discovery of a meagre pamphlet of the eighteenth century, only two or three copies of which now exist, we find ourselves forced to change the generic names of common birds, familiar as they are by long and constant usage.

In the discussion of these questions all personal considerations should be entirely rejected. The smallest interest or convenience to the science in general, followed as it is by a republic of thousands, is of more importance than any compliment to the feelings of a living, or the memory of a deceased naturalist. In fact our mere recognition of an author's names is not of such vast importance to his reputation. His fame must rest upon a securer foundation than this. For the custom of placing the name of an author after a species described by him is not (or should not be) done for that author's personal advantage, but simply to assist us in the recognition of that species. It is a short method of referring to the place where the description of the species may be found, or enables us to distinguish it from some other to which the same name has been by mistake applied; as, *Pleurotoma violacea*, Hinds, *non* Mighels. In this view, how ludicrous it appears, to hear, as we often do, naturalists complain that if the custom of placing after a species the name of that author who first placed it in its proper genus is adhered to, more than one-half of Linné's species will be wrested from him. Does the fame of the great Linnæus depend upon the number of species he described?

We will now mention a few points concerning which great difference of opinion exists in the minds of naturalists, and which for the good of science should be immediately settled in one way or the other. The first is: shall the same generic name be allowed to occur in different departments of zoology or botany, or even in both these, or, we may add, in other sciences. Many are of the opinion that they may be used, and should not be changed, if so occurring;—in view of the great difficulty now experienced in selecting a name which is not preoccupied, and shall be at the same time descriptive or suggestive of the object intended. But what is the object of a name? Surely, the *main* object is to enable us to distinguish one thing from another, and from all others, that when it is used we may know what is intended, and not be forced to decide by other aids. Is it not of vastly more importance that a name should serve this purpose, than that it should remotely indicate (which is the most generally possible) some character of the object, which it may after all hold in common with an hundred others? Greek compounds are by no means exhausted yet, and if they were, we might fall back upon euphonic names, which serve the purpose however barbarous they may appear in the eyes of some. The custom of using the same name for many diverse objects is productive of serious inconveniences. If we have stars, countries, minerals, plants, vertebrates, articulates, mollusks and radiates, all named alike, some singular anomalies might occur, since we can of course reduplicate *specific* appellations as often as we please in different genera. For instance, suppose a travelling naturalist "making his researches in Arizona, observed specimens of the *Arizona pætula* (hermit-crab) inhabiting the shell of *Arizona pætula* (univalve), creeping among the roots of *Arizona pætula* (shrub); and upon examining it anatomically, found great numbers of the *Arizona pætula* (infusorium) living in its gills. The *Arizona pætula* (bird) was feeding upon these crabs with great voracity," etc.

Another point. A genus may contain a vast number of species, and yet from want of profound investigations no one may see the propriety

of dividing it up. As occurs very commonly, in the course of time some new species belonging to it are described under names, which, being preoccupied in that genus, are very properly changed. The new designations become established and may be used for years. At last it becomes necessary to divide the genus, and the species whose names have been referred to are found to belong to different genera. Shall the old reduplicated specific name, or the substituted one be now adhered to? Naturalists are about equally divided in opinion upon this point.

The propriety of using small initial letters to proper specific names, nouns or adjectives, has been made the subject of discussion. Whatever method be followed here, it would seem that uniformity is desirable; if any of these proper names are to have small initials, why not all? Most zoologists and botanists seem in this matter to follow the usage of their own language rather than that of the Latin, or any uniform system. The Germans will have all nouns begin with a capital, and all adjectives with a small letter, as *Ocypode Cursor*, *Chiton emersonianus*, whereas the English write common nouns with a small initial, and all *proper* appellations, whether nouns or adjectives, with a capital, as *Ocypode cursor*, *Chiton Emersonianus*. The truly convenient system will be to write all specific names without exception, with a small initial letter, as is done by one of the most eminent zoologists of this country, and by many of those of Europe. We shall then have no difficulty in distinguishing specific from generic names, and may discuss the relations of species without the necessity of repeating the generic name or its initial every time they are mentioned. A proper name, modified for use as a specific appellation, becomes a part of a new title, and involves a different idea.

We will not detain the reader by discussing other mooted points, as whether ante-Linnæan names shall be accepted, if binomial; whether names of faulty etymology shall be corrected, etc. The above are only mentioned as instances of the necessity of establishing many rules to produce uniformity of usage among naturalists. In pointing out how this may be satisfactorily done, we proceed to our promised suggestion.

We have somewhere read, that when any orthographical or other difficulty occurs in the use of the French language, the case is immediately referred,—in accordance with the admirable *system* for which the nation are remarkable both in science and literature,—to the Academy, who decide upon it, arbitrarily it may be, but finally. The action of this tribunal is respected, and no farther uncertainty or diversity in the use or spelling of the word can occur to embarrass French authors. Now why may not a similar mode of action be of use in science, and enable us at last to settle all our difficulties. Science is cosmopolitan, not national. Let a convention meet at Paris or some other central point, composed of delegates from all the scientific societies of the earth, and representing at least all the departments of zoology and botany. Here they may hold sessions of the entire body, for a sufficient length of time to establish all the general rules of nomenclature which can be conveniently applied. But as we have before observed, there are some particular cases for which no rule will serve, and which must in some way be decided separately and *arbitrarily*. For the settlement of these cases let the convention divide itself into as many sections or committees as there are classes of

plants and animals; a committee of ornithologists for the birds, of entomologists for the insects, etc. These committees being composed of experts in the various branches, will not find it difficult to discuss and pass judgment by vote upon the name of each contested genus or species. Let these decisions be respected, and let the names of those who will not abide by them be placed upon a new edition of that black-book which LINNÆUS kept of old,—the list of *Damnati!*

2. *Les genres Loriope et Peltogaster*, H. Rathke; par W. LILJEBORG, Professeur de Zoologie à Upsala en Svède. pp. 35, 4to, pl. 3. (Extr. des Nova Acta Reg. Soc. Sc. Upsal, Ser. 3, Vol. iii.)—M. Liljeborg has unraveled a singular history in relation to the curious sac-like parasites found on the abdomens of Decapod crustaceans. They were first observed by Cavolini, and in more recent times by Rathke, who considered them to be worms, probably Entozoa, and instituted the genus *Peltogaster* for their reception, describing two species, *P. paguri* and *P. carcini*. Diesing placed them among his Myzelmintha, and formed a new genus, *Pachybdella*, for *P. carcini*. These forms were made the subject of discussion by Kroyer, Steenstrup, O. Schmidt, and Lindstrom, who agreed with Cavolini in referring them to the Crustacea, but could only conjecture their more intimate relations, although suggesting those with the Entomostraca, the Lerneidæ, or the Bopyridæ. From a study of their larvæ M. Liljeborg now ascertains their true place to be among the Cirripedes, and describes two new species.

Within the body of *Peltogaster paguri*, Rathke found a minute Tetradecapod, scarcely a line in length, which he considered to have been swallowed as food by that animal, and described it as an Amphipod under the name of *Loriope*. It was afterward referred to the Tanaidæ by Dana, who described a new allied genus *Cryptothir*. Its history however remained very obscure, although it was demonstrated that, being always found alive, it was not the food of the *Peltogaster*, and some naturalists even suspected it to be the male of that parasite. By a fortunate discovery M. Liljeborg has now cleared up the difficulty. In examining a *Peltogaster* taken from the abdomen of *Pagurus pubescens*, he found attached to it another sac-like body filled with Loriopes, which might well have been taken for the egg-pouch of the *Peltogaster* itself, but which after careful study proved to be a distinct animal,—a parasite upon a parasite! It proved, in fact, to be the female of the *Loriope*, grown monstrous by a process of degradation similar to that observed in the female of the *Bopyridæ*, to which family indeed the *Loriope* must now be referred. The occurrence of the young Loriopes in the digestive cavity of *Peltogaster* is, then, simply adventitious.

3. *Neue Wirbellose Thiere, beobachtet und gesammelt auf einer Reise um die Erde*, 1853 bis 1857, von LUDWIG K. SCHMARDA; 1st Band, 1st Hälfte, 4to, pp. 66, and 15 colored plates. Leipzig, 1859. (New York, B. Westermann & Co.)—A quarto volume, handsomely bound and splendidly illustrated, containing descriptions of the Turbellaria and Rotatoria collected and observed by Dr. Schmarda during a voyage round the world. The descriptions of the animals belonging to the former order are made additionally clear by woodcuts showing the shape of the head and the distribution of the ocelli. A review is given of the genera of

each order. The Dendrocœla are divided into two divisions, *Acarena* and *Carenota*, the former having no distinct head, which those of the latter group possess. We should judge this to be a character of much less importance than those derived from anatomical characters, which forbid such a division. Following it, the author separates *Planaria gonocephala* from the other freshwater Planariæ and places it with such forms as *Cephalolepta* and *Planaria oceanica*, Darwin! There is also a large number of genera hitherto considered sufficiently well established, which are entirely ignored by M. Schmarda, as *Prosthiostomum*, *Dendrocœlum*, *Procerodes*, *Fovia*, *Bdelloura*, *Geoplana*, and *Rhynchodemus*. The new genera of Dendrocœla are *Dicelis*, *Prostheceræus*, *Homaloceræus*, *Goniocarena*, *Carenoceræus*, and *Sphyrocephalus*. Of these several have been previously established under different names. *Prostheceræus* may be adopted, as Quatrefages' name *Proceros* is preoccupied. *Goniocarena* is *Dugesia* Girard, *Carenoceræus* is *Nautiloplana* Stm., and *Sphyrocephalus* is *Bipalium*.

The Nemertinea are subdivided upon more certain grounds than the preceding order, but we are at a loss to understand why the proboscis should not be considered as the mouth, as it is certainly the aperture through which food is introduced into the body. Oken's name *Borlasia* is adopted (although it is exactly synonymous with *Lineus*, Sow.) and made to include *Amphiporus*, *Acrostomum* and *Baseodiscus* of Diesing. A new genus, *Loxorrhochma*, is established for the *Polia coronata* of Quatrefages. The figures are excellent, showing well the colors, which among these animals form the most reliable specific characters, and notwithstanding errors of nomenclature and arrangement, we have to thank M. Schmarda for an exceedingly interesting and beautiful work. W. S.

4. *A Supplement to the "Terrestrial Air-breathing Mollusks of the United States and the Adjacent Territories of North America;"* by W. G. Binney. Boston, 1859. 8vo, pp. 207, and 6 colored plates. (Extracted from the Journal of the Boston Society of Natural History.)—This much needed supplement to Dr. Binney's great work contains the additions and corrections which have accumulated in the rapid progress of the science during the seven years which have elapsed since the publication of that work. Several doubtful species which were omitted in that work have since been investigated, and are now included or referred to their proper place in the synonymy. Besides these, we have figures and descriptions of all the recently discovered species, of which the number is large, particularly among those of the Pacific coast. These are placed together in the first part of the volume. The writings of foreign authors upon our land-snails are properly discussed, and their descriptions reprinted in full wherever there was any doubt as to the species to which they should be referred. Thus the whole subject is thoroughly posted up to the date of Jan. 1st, 1859, and the work forms a most acceptable contribution to American conchology. W. S.

5. *Catalogue of the Recent Marine Shells found on the Coasts of North and South Carolina;* by J. D. Kurtz. 8vo, pp. 10. Portland, Me., 1860.—This catalogue shows the results of the author's researches in the conchological fauna of our southern coast made in the years 1848–52. The number of species given is 204, an increase of 78 over that given

by Prof. Gibbes in his catalogue published in Tuomey's "Geology of South Carolina," published in 1848. Several species are mentioned as occurring on the marl-bottom off the N. C. coast which have not been hitherto observed north of Florida. Four new species are briefly described, viz., *Venus trapezoidalis*, (which is perhaps the same as *V. pygmæa*,) *Arca Holmesii*, *Scalaria rupicola* and *Chemnitzia textilis*; and several new ones are mentioned by name only. Capt. Kurtz has also contributed very largely to our knowledge of the marine animals of the same coasts, in other departments, as our zoological archives abundantly show. W. S.

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 281, Resolutions on the death of Thomas Nuttall.—A new Unio from the Isthmus of Darien; *I. Lea.*—Additions to the Coleopterous Fauna of Northern California and Oregon; *J. L. LeConte.*—p. 294, Notes and Descriptions of Foreign Reptiles; *E. D. Cope.*—p. 297, A new Myalina and Posidonia from the Carboniferous of Texas; *W. M. Gabb.*—p. 297, New birds from Cape St. Lucas; *J. Xantus.*—p. 299, Notes on Birds collected at Cape St. Lucas by Mr. John Xantus; *S. F. Baird.*—p. 306, Mineralogical Notices; *W. J. Taylor.*—p. 310, New Histeridæ; *J. Leconte.*—p. 317, Contributions to American Lepidopterology, No. 2; *B. Clemens.*—p. 329, On the soft parts of certain Unionidæ; *I. Lea.*—p. 331, Descriptions of three new species of exotic Uniones; *I. Lea.*—Notice of Shells collected by Xantus at Cape St. Lucas: *P. P. Carpenter.*—p. 332, Catalogue of the Venomous Serpents in the Museum of the Academy, with notes on the families, genera, and species (new genus, Teleuraspis); *E. D. Cope.*—Index to vol. for 1859.

Transactions of the Am. Philos. Society (*Philadelphia*). Vol. XII (new series), Part II, contains:—Art. XI. Della Correlazione delle Forze Chimiche Colla Rifrangibitita delle Irradiazione; Di Zantedeschi.—Esperimenti Eeguiti Col Calorico Solave.—Art. XII. Geological sketch of the estuary and freshwater deposit of the Bad Lands of the Judith, with some remarks upon the surrounding formations; by F. V. Hayden, M.D.—Art. XIII. Extinct Vertebrata from the Judith River and Great Lignite Formations of Nebraska; by Joseph Leidy, M.D.—Art. XIV. A sketch of the Botany of the Basin of the great Salt Lake of Utah; by E. Durand.—Art. XV. Observations on the Magnetic Dip in the United States; by Prof. Elias Loomis.

Report of the Twenty-eighth Meeting of the British Association for the Advancement of Science, held at Leeds, Sept. 1858. London, 1859.—Contains the following zoological papers of more general interest:—On the Anatomy of the Araneidea, particularly of their Spinning organs (2 plates); *R. H. Meade.*—Various reports of Dredging committees.—On the reproductive organs of Sertularia tamarisca; *Allman.*—On the Migration of Birds; *Collingwood.*—Anatomy of the Brain in some small quadrupeds; *Garner.*—On the formation of the Cells of Bees; *Tegetmeier.*

Det Kongelige Danske Videnskabernes Selskabs Skrifter, 5te Raekke, Naturv. og Math. Afdeling, 4de Binds. Copenhagen, 1856-59.—Contains the following zoological papers:—Coroctoca and Spirachtha, Staphylines which bring forth living young and are domesticated with a Termite (with 2 plates); *J. C. Shiödte.*—On the Hectocotyle forms of the Octopod genera Argonauta and Tremoctopus (with 2 plates); *J. J. Sm. Steenstrup.*—Attempt at a monographic exposition of Sergestes, a genus of Shrimps, with remarks upon the organs of hearing in the Decapod Crustacea (5 plates); *H. Kroyer.*—On Mephitis Westermanni, a new "Stinkdyr" from Brazil; *J. Reinhardt.*—5te Binds, 1ste Hefte, 1859: Additamenta ad historiam Ophiuridarum,—descriptions of new or little known species of Serpent-stars; 1st and 2d parts, 7 plates, (new genera, Ophiocten, Ophionereis, Ophiactis, Ophiostigma, and Ophioblenna); *Chr. Fr. Lütken.* W. S.

6. *New Zoological Journal.*—Dr. H. F. Weinland, already well known to American zoologists, has commenced, Oct. 1, 1859, at Frankfort on the Maine, a monthly journal (8vo, 16 pp.) entitled *Der Zoologische Carten. Organ für die Zoologische Gesellschaft.*

IV. ASTRONOMY AND METEOROLOGY.

1. *Supposed intra-Mercurial planet.*—The announcement of M. Le Verrier that the existence of one or more planets within the orbit of Mercury is rendered highly probable by his computations on the movement of the perihelion of Mercury, has called out former observations of the transits of bodies over the sun's disc, in addition to those mentioned in vol. xxviii, pp. 445 and 446 of this Journal.

(1.) Dr. Lescarbault at Orgères (Dept. Eure et-Loir), France, observed with a telescope, March 26th, 1859, a small black circular spot moving across the upper limb of the sun, at a rate which would occupy $4^h\ 26^m\ 48^s$ to traverse the entire disc. Its apparent diameter was less than a quarter that of Mercury in transit. M. LeVerrier thinks the observation worthy of credit, and computes that on the supposition of a circular orbit the time of the planet's revolution around the sun is $19^d{\cdot}7$, and the inclination of its orbit $12°\ 10'$. Its greatest elongation from the sun would not exceed eight degrees, and its light be less than that of Mercury. This planet seems however insufficient to account for all the movement of the perihelion of Mercury.—*Comptes Rend. Acad. Sci., Jan.* 2, 1860.

(2.) Mr. Benj. Scott, of London, states that about midsummer in 1847, he chanced to turn a telescope towards the sun then near its setting, and saw on the sun's disc a well defined black spot, which was not to be seen there at sunrise the next morning. Its angular diameter appeared as large as that of Venus. Mr. Scott mentions that a similar body, or spot, was seen by Mr. Lloft, January 6, 1818.

2. *Mr. Alvan Clark's New Micrometer for measuring large Distances,* (Extracted from the Monthly Notices of the Royal Astronomical Society for July 1859).—At the monthly meeting of the Society in June, Mr. Alvan Clark, of Boston U. S., exhibited a micrometer invented by himself, which is capable of measuring with accuracy any distance up to about one degree. It is also furnished with a position-circle. Its character is essentially the same as that of the parallel-wire micrometer; but it has some peculiarities not, it is believed, previously introduced, and on which its wide range depends.

The most remarkable of these peculiarities consists in its being furnished with *two eye-pieces*, composed of small single lenses, mounted in separate frames, which slide in a grove, and can be separated to the required distance. A frame carrying two parallel spider-lines, each mounted separately with its own micrometer-screw, slides in a dove tailed grove in front of the eye-pieces; and, by a free motion in this frame, each web can be brought opposite to its own eye-lens.

In using this micrometer, the first step is, to set the position-vernier to the approximate position of the objects to be measured. Then the eye-lenses are separated till each is opposite to its own object. The frame containing the webs and their micrometer-screws is then slid into its place; and, the webs having been separated nearly to their proper distance by their free motion in the frame, they are placed precisely on the objects by their fine screws, the observer's eye being carried rapidly from one eye-lens to the other a few times, till he is satisfied of the bisection of each of the objects by its own web. The frame is then removed for

reading off the measure by means of an achromatic microscope, on the stage of which it is placed. One of the webs is brought to the intersection cross-wires in the eye-piece of the microscope; and by turning a screw (the revolutions of which are counted), the frame travels before the microscope, and the other web is brought to the intersection of the cross-wires. The *parts* of a revolution are read off by a vernier from a large divided circle attached to the screw.

The advantages arising from the peculiar construction of this micrometer are the following:—

1. Distances can be observed with great accuracy up to about *one degree*, and the angles of position also.

2. The webs, being in the same plane, are free from parallax, and are both equally distinct, however high the magnifying power may be.

3. The webs are also free from distortion and from color.

4. A different magnifying power may be used on each of the objects; which may be advantageous in comparing a faint comet with a star.

3. *New Double Stars discovered by Mr. Alvan Clark;* communicated by the Rev. W. R. Dawes. (From Monthly Notices of the Royal Astronomical Society, xx., p. 55. Second series.)

Curr't No.	Designation.	R.A. 1860.	N.P.D. 1860.	Mag.	Dist.	Date of discovery.
		h m s	° ′		″	
13	Lalande 1980	1 0 51	45 32·4	8, 8	0·45	1858, Nov. 29
14	Lalande 2634	1 20 7	47 56·6	8, 9	0·8	1858, Oct. 13
15	99 Herculis	18 1 43	59 27·4	5½, 10½	1·7	1859, July 10
16	* Vulpeculæ	19 52	63 7	7½, 8	0·4	1858, Aug. 30
17	{ Cygni 153 B / B.A.C. 6959 }	20 8 3·8	38 57·4	6, 11½	3·8	1859, July 19
18	44 Cygni	20 25 41	53 32·0	6½, 11½	2·8	1859, July 6
19	A.Z. xxiv. 11	21 10 50	26 10·2	7½, 7½	+0·9	1859, July 8
20	75 Cygni	21 34 41	47 21·6	5½, 11	2·9	1859, July 19

All the double stars in this series may be considered as good test-objects for telescopes of from 6 to 8 or 9 inches aperture. But it is not merely as such that they are interesting: they become especially so from the fact that they are all situated in the northern hemisphere, and all but one at more than 30° from the equator. They consequently attain a good altitude at Pulkova; notwithstanding which they have escaped the acute search of M. Otto Struve with the 15-inch refractor, in addition to the previous one by his father with the Dorpat telescope of 9·6 inches aperture. Either, therefore, they have recently undergone considerable change; or, if not, it appears that objects of great difficulty and delicacy may be detected with very perfect telescopes of smaller size, which have escaped the most diligent scrutiny with far larger instruments. The whole of these objects were discovered by Mr. Clark with his own object-glasses of 8 and 8¼ inches aperture, and five of them in my own observatory, during his visit to me last summer; since which I have met with *seven others*, of similar character and situation; and it should be mentioned that none of the objects recorded are below Struve's eighth magnitude for the larger star; all smaller, of which many have been found, having been systematically rejected. It appears, therefore, that there is still much oc-

cupation for telescopes of moderate dimensions, even in this department of astronomy, which might reasonably have been supposed to have been long since exhausted. The *distances* stated in the list are from my own recent measurements.

Haddenham, Thame, November, 1859.

4. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—In the last No. of this Journal, p. 137, I gave a brief notice of this meteor, but from want of space was compelled to limit myself to a brief summary of results. I have received a large amount of documents relating to this meteor, most of which however are too indefinite to be of much value. I now proceed to present a brief summary of what appear to me the most reliable observations.

At New Haven, Ct., Judge W. W. Boardman saw the meteor descend at an angle of 25° to 35° with the vertical, and it passed from his view at the edge of the dome of a steeple in azimuth S. 35° 34′ W. Continuing the meteor's path down to an altitude of 3° or 2°, we have S. 37° W. for the azimuth of the place where the meteor would have disappeared to him had his view been unobstructed.

At New York city Mr. Tatham was riding in the Bowery, and saw the meteor descend at an angle of 20° with the vertical, and in a range with the middle of the street opposite the Old Bowery theatre. According to the map of the city, this direction was S. $27\frac{1}{2}$° W. The diameter of the meteor appeared to be about one-third that of the full moon.

A correspondent of the Evening Post, walking down Broadway, saw the meteor disappear in azimuth S. 25° W.

Mr. Gould, also in Broadway, saw the meteor disappear behind a building in azimuth S. $23\frac{1}{2}$° W.

Mr. Pirsson, also in Broadway, saw the meteor disappear behind a high building in azimuth S. 21° W.

Mr. Bradley, also in Broadway, reports that the meteor disappeared in azimuth S. 16° W.

Several other observers agree as to the general direction of the meteor, but their statements are less precise than those of the preceding. As Mr. Bradley's observation differs materially from the others, I reject it, presuming that his memory must have been in fault, either in respect to his point of observation, or that of the meteor's disappearance. The mean of the other four estimates is S. $24\frac{1}{4}$° W., or allowing for the effect of the high buildings which obstructed the view of three of the observers, the mean would be about S. 26° W. This result differs five degrees from my former estimate; a difference which is explained by my having obtained two new observations, and by my rejection of Mr. Bradley's observation.

At Washington, the apparent path of the meteor was vertical, and its point of disappearance was estimated at four degrees north of east.

A gentleman four miles west of Dover, Del., was riding towards Dover. His wife saw the meteor; he only saw the smoky trail which he describes as a nearly vertical column, with its base 20°, and its top 40° from the horizon; direction due east.

Mr. Parsons, at Salisbury, Somerset county, Md., saw the meteor descend in a slanting direction to the earth, when it exploded with a dull sound. Its direction was from the N.E.

At Lewistown, Del., the meteor was seen to fall in the N.E. The report was heard five minutes later—loud but distant.

If we mark upon a map all the preceding directions, we find that the lines do not intersect at one point, but they indicate the most probable point of the meteor's disappearance to have been near lat. 39° 10′ and long. 75° 5′.

At New Haven, the path of the meteor was estimated to make an angle of from 25° to 35° with the vertical. Mr. Wilder Smith, near Waterbury, Ct., estimated the inclination to the vertical at about 30°.

At New York, Mr. Tatham estimated the angle with the vertical at 20°, Mr. Gould 10°, Mr. Pirsson 35°, and Mr. Bradley 45°. The mean of these four estimates is 27°.

At Washington, the path was pronounced exactly vertical. The actual path of the meteor was therefore such as, if continued, must undoubtedly have struck the earth. It must have passed vertically over the extreme southern part of New Jersey, and must have struck the earth in Delaware Bay, or near its shore.

That this conclusion is a near approximation to the truth, is confirmed by observations from the southern part of New Jersey.

Mr. Mills was surveying in the forest four miles west of Stephens Creek in Atlantic county, and heard a noise nearly overhead. He looked up and saw a cloud of a rounded form like a puff of smoke about 15 degrees south of the zenith.

At Millville, Cumberland county, a strange rumbling noise was heard somewhat resembling thunder, and one or more clouds of smoke were seen in a southeast direction at an elevation very roughly estimated at 45°.

At Newport, Cumberland county, a rumbling noise, which lasted two minutes, was heard in an east or southeast direction.

At Maurice River Cove, Cumberland county, the captains of the oyster boats saw a flash and smoke in an easterly direction.

At Dias Creek, Cape May county, Mr. Smith states that the noise was great and lasted two or three minutes. The flash was brilliant, and the smoke was seen in a northeast direction at an elevation of 75° or 80° above the horizon.

At Goshen, Cape May county, a noise was heard in a northeast direction, and a cloud of a rounded form was seen in the northeast.

At Dennisville, Cape May county, the noise appeared directly overhead. There was a small cloud or belt of white smoke left in the train of the meteor, about five degrees northwest of the zenith, the atmosphere being perfectly clear at the time. The detonations lasted somewhat over a minute.

The directions indicated in the preceding notices have a decided convergence towards a point near lat. 39° 13′, and long. 74° 52′. This result accords so nearly with that derived from observations made at a distance of a hundred miles and upwards, as to show that the observations are in the main reliable, but subject to that uncertainty which attends all estimates made without instruments, and not reduced immediately to writing. We must then conclude that this meteor passed vertically over the southern part of New Jersey, nearly on the parallel of 39° 13′, and that it struck the earth near the eastern shore of Delaware Bay, probably between Dennis Creek and Maurice River.

I assume that this meteor was a solid body. We are acquainted with two classes of meteors quite distinct from each other and differing greatly in density. Ordinary shooting stars have never been known to reach the earth's surface, or to produce any audible noise. Another class of meteors like that of Agram in 1751 is composed chiefly of iron, and another like that of Weston, Ct. (1807), partly of iron and partly of silicates. They frequently strike the earth, or at least let fall fragments to the earth, and are attended not only by a brilliant flash of light, but by a tremendous noise. The New Jersey meteor bore a striking resemblance to the Weston meteor, not only in the brilliancy of its light, but in the noise which attended it. We cannot doubt that it was a body of considerable density; and the direction of its motion was such that the entire mass must have struck the earth. It may have sunk into Delaware Bay and not a single fragment have fallen upon dry land; but there is reason to hope that at least some fragments of it may yet be discovered. Such fragments, if they exist, are probably scattered along an east and west line coinciding nearly with the parallel of 39° 13′, and the entire mass probably lies near the meridian of 75°.

5. *Sandwich Island Meteor of Nov.* 14, 1859.—A meteor of remarkable size and brilliancy was seen from the slope of Mauna Kea north of the great volcanoe of Kilauea, S. I., soon after dark of the 14th November last, shaped like a cross, having the light of the moon at full, moving vertically south from a point a little below the zenith and disappearing near the crater.—*Pacific Com. Advertiser, Dec.* 15.

6. *Der Meteoreisenfall von Hraschina bei Agram am* 26 *Mai* 1751; von W. Haidinger. Wien, 1859.—Prof. Haidinger has here revised all the contemporary evidence respecting the fall of this remarkable meteoric iron mass, the details of which are fortunately well authenticated. This history is of great interest at the present moment when the late meteors of August and November last have called up anew the discussion of this subject.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Monthly varying level of Lake Ontario, measured, in inches, from a fixed point above the surface downwards, for fourteen years, at Charlotte, mouth of Genesee River, N. Y.:*

Year.	Jan	Feb	Mar	Apr	May	Jun	July	Aug	Sep	Oct	Nov	Dec	Mean	Range	
1846	51	54	48	45	42	39	39	42	45	45	48	45	45·2	15	Low.
1847	48	42	36	36	29	25	25	25	36	39	43	46	35·8	23	Higher.
1848	29	34	43	38	38	37	38	39	44	49	54	53	41·3	25	Lower.
1849	50	50	52	46	36	33	44	39	45	38	38	41	42·7	19	do.
1850	45	40	40	40	32	29	34	46	47	52	55	43	41·9	26	do.
1851	44	54	48	47	44	38	35	38	42	47	53	51	45·0	19	Low.
1852	50	51	48	44	26	26	22	24	30	23	38	34	34·7	29	Higher.
1853	35	32	32	25	20	14	27	20	24	28	38	39	27·8	25	Mean.
1854	39	39	38	38	27	24	25	27	36	44	48	50	36·2	26	Lower.
1855	52	53	36	40	40	36	34	36	36	34	33	33	38·6	20	do.
1856	35	35	33	31	23	18	28	30	37	46	53	53	34·7	35	Higher.
1857	54	56	46	44	35	24	19	12	14	9	24	22	30·0	47	Mean.
1858	19	13	13	18	4	6	4	2	8	12	14	16	10·9	17	Very high.
1859	20	24	16	10	6	2	8	11	17	22	28	25	15·7	26	Lower.
Mean.	41	41	38	36	29	25	27	28	33	35	40	39	34·3	25	

1. As more water falls usually in the warmer months, the Lake is higher in those months generally than in the colder months.

2. The range has been only 54 inches, the lowest being in February, 1857, and the highest in August, 1858, and in June, 1359; the mean of the two is 27 inches.

3. In 1846 and 1857 the mean level *lowest*, and in 1858 and 1859 *highest*.

4. In 1853 the Lake was near the *mean level*, and in 1857 only a little less, though the first half of the year gave low water and the last half high.

5. The Lake was near the highest, or within four inches of it, in May, June, July and August, 1858, and in May and June, 1859, and of course the average of both years was high.

6. The Lake down to 50 inches or more in January and February, 1846; in November and December, 1848; in January, February and March, 1849; in October and November, 1850; in February, November and December, 1851; in January and February, 1852; in December, 1854; in January and February, 1855; in November and December, 1856; and in January and February, 1857.

These statements show that the changes of the level must be owing to the ordinary causes of supply or diminution of water over this great watershed, and disprove any notion of *periodic* rise and fall under any but meteoric laws. As the water was high in the Lake through 1858, it was suggested that the average fall of water must continue it high in 1859, as the measures now prove. In November, 1859, the water fell to the mean, and rose afterwards from the great autumnal rains at the west which had flowed into Lake Ontario. C. D.

2. *Eruption of Mauna Loa, Sandwich Islands*, (in a letter to Prof. DANA from Prof. R. C. HASKELL, Oahu College, dated Honolulu, Nov. 5, 1859).—Since my last dates (June 22d)* the lava continues to flow from the place of the recent eruption. With scarcely any cessation since the middle of June it has been flowing into the sea. Hawaii has been increased in area by many acres at least, by several hundred acres it is said.

After writing you from Kona in June, I visited Kilauea, which I found *very quiet*. There has, however, been considerable action since you were there in 1840, for the crater is now filled up even with the "black ledge" of which Wilkes speaks.

From Kilauea, passing through Hilo, I went to Waimeu, intending to ascend Mauna Kea, but the weather proved so rainy and foggy that I was unable to do so. From Waimeu I went direct to Kona, crossing the lava stream without difficulty on a mule, between the three mountains. The stream was fully three miles wide where I crossed, and at some points above appeared to be five or six miles wide. At this time the lava was flowing into the sea, and of course running under me as I crossed, yet the lava on the surface was in no place so hot as to burn the hoofs of the mule, or even to be noticed by myself, unless I touched my hand to it.

After arriving at Kona I went by canoe to visit the place where the lava was then and is still flowing into the sea. Without attempting to give an adequate description of the sight presented as I passed, by night, a few rods in front of the stream, which was more than a mile wide, I will only mention one fact.

* Vol. xxviii, [2], 284.

The lava was at a light-red heat, and flowed into the sea with a velocity of two or three miles per hour. And yet this point is forty miles from the source of the stream, and at least twenty-five miles from the lowest point to which the "fissure" in Mauna Loa can possibly extend. Therefore the lava flows twenty-five miles at least, without receiving any heat from the interior of the earth, and yet is still of a light-red heat. It will be remembered, of course, that the stream is covered over with solid lava all the way from the source to within a few feet of the sea, with the exception of a small opening here and there, once in a mile perhaps.

Rev. T. Coan adds, under date of Hilo, Hawaii, Nov. 25, 1859.—"The old lake of fusion in Kilauea is slowly enlarging, and the area around it is subsiding. Probably it may in time resume its old size of half a mile in diameter. Recent visitors have found it active. On one occasion it was thought to throw up jets to the height of 70 feet.

The present eruption has now been in progress ten months, and our last advices report it still active. Several streams have fallen into the ocean along the coast of Kona. These are of different widths, and some of them are separated miles from each other. A small village, Kibele, has been covered of late with the lava, and a large and valuable fish-pond filled up. The people in Kibele pulled down their houses, and also the church, on the approach of the lava stream, and carried off the materials. Just above the church the fiery stream parted, flowed along on each side of the ground where the church had stood, reunited below it, and continued in one stream to the sea. This fact struck the Hawaiians as marvellous, and they regretted having removed their house of worship.

During the early stages of this eruption there were many splendid exhibitions along the line of flow. Canals, cataracts, lakes, fountains and jets of fusion were seen along the slope of the mountain. Forests were consumed, rocks were rent, loud and startling detonations were heard, and the heavens were shrouded with a pall of darkness. Now, and for a long season past, little or no fire is seen, except where the red lava pours into the sea. Here a broken line of fusion is seen coming out from under its self-made counterpane of hardened lava, and pouring down the face of a low and cragged precipice into the ocean, keeping up a constant boiling and sending up clouds of vapor into the air.

The central parts of Kilauea are more quiet than any other part of the crater. We have occasional earthquakes. Two shocks occurred in February, one in July, and two in November of the current year."

Book Notices.—

1. *Trübner's Bibliographical Guide to American Literature:* a classed list of books, published in the United States of America during the last forty years. With Bibliographical Introduction, Notes and Alphabetical Index. Compiled and edited by Nicholas Trübner. London, 1859. 8vo, 554 pp.—This work is beyond all question the best guide which we have to recent American literature and science. Not only is it better than all other bibliographical works of a similar scope but it is excellent in itself. Our limits permit us to mention only one of its most valuable features. Special attention has been bestowed on works in natural science, not only those which appear with an author's name, and are accordingly easy to trace, but more particularly on serial works, such as scientific journals, the transactions of learned societies, and reports of the

state and national legislatures, which are often very difficult to discover by the ordinary apparatus of the trade. Mr. Trübner not only mentions what constitute complete sets of such works; but he enumerates the contents of the several volumes,—so that by means of his excellent index a multitude of articles and essays, often overlooked, are brought to the knowledge of every student. This book should be owned by every book-buyer. D. C. G.

2. *Manual of Public Libraries, Institutions and Societies in the United States and British Provinces of North America;* by WILLIAM J. RHEES, chief clerk of the Smithsonian Institution. 8vo, pp. 687. Lippincott. Philadelphia, 1859.—This volume contains a great amount of useful information on Public Libraries, and gives evidence of much labor in its compilation. The list of libraries in the various States extends to over three thousand titles. In a second edition the Author will be able to supply some obvious deficiencies which are inseparable from the first cast of such a work.

3. *The New American Cyclopedia: a popular Dictionary of general Knowledge;* edited by GEO. RIPLEY and CHAS. A. DANA. Vol. I—VIII. 8vo. New York and London: D. Appleton & Co.—Since our former notice of this Cyclopedia it has advanced rapidly, until now we have before us eight volumes of eight hundred pages each, the last article being on the too famous HAYNAU. Such promptness in issuing so large a mass of elaborately prepared matter speaks well not only for the energy of the publishers and the industry of its editors, but also of the public appreciation of the work. Like its predecessor, the "*Encyclopedia Americana*," 1829–47, by Dr. LIEBER and others, it gives a satisfactory response to almost all questions coming within the range of its plan. The *New Cyclopedia*, however, besides its greater range of topics, has the advantage derived from a vast progress in many departments of knowledge, developed by a numerous corps of contributors skilled each in his own speciality.

In looking over its articles with a peculiar reference to our own departments, we are often tempted to linger among its miscellaneous topics, so rich in various and interesting information. The fine arts, religion, law, politics and war, share our attention with history and biography, ancient and modern, foreign and American, including persons still living, with sketches of events within their respective eras; geography, with the physical and picturesque features and the mineral treasures of particular countries; common and useful arts, agriculture, mechanics, and their various productions; gas lighting, gun-powder, its history, manufacture and uses; caoutchouc, gutta percha, and kindred topics of technical chemistry, with their diversified applications, and a multitude of other subjects, more or less practical and interesting to society at large.

In the American Cyclopedia the articles on science are numerous and valuable, and elevate the work to the character of a compendium of modern science. These articles are in most cases written with decided ability, and evidently by persons who are familiar with the topics which they discuss. While many of the less important subjects are presented with luminous brevity, others are more fully expanded. Among these are many topics of natural history; Chemistry is presented with its equivalents and laws of combination illustrated by many of its modern discoveries and practical applications; of the latter an example is found in the full account of the manufacture of gelatine, of beer and bricks, and

in the ample history of gas lighting. Geology, voltaic electricity, magnetism, and other departments of pure or applied science, are treated with reasonable fulness. As a literary work the Cyclopedia is written in a pure and chaste style, and exhibits the candor and fairness which should ever adorn a record of universal knowledge. B. S.

An Introduction to Practical Pharmacy: designed as a text-book for the Student, &c.; by EDWARD PARRISH. Philadelphia. 2d edition. 246 illustrations. 8vo, pp. 720. Blanchard & Lea, 1859.—A well arranged and carefully prepared treatise, adapted to the state of this art in the United States.

Elements of Inorganic Chemistry, including the applications of the Science in the Arts; by THOS. GRAHAM, F.R.S., L. and E. Edited by HENRY WATTS, B.A., and ROBT. BRIDGES, M.D. 2d American edition in one volume. 233 woodcuts, 8vo, pp. 852. Philadelphia: Blanchard & Lea. 1858.—The American publishers issued the 430 first pages of this volume in 1852 under the editorship of Dr. Bridges. The remainder is reproduced without alteration from the English edition.

Nautical Monographs, No. I. (Washington Observatory) Oct. 1859. The winds at sea, their mean direction, and annual average duration from each of the four quarters. With four plates of diagrams of winds and calms. 4to, pp. 8. By Lt. M. F. MAURY.

Caloric: its Mechanical, Chemical and Vital Agencies in the Phenomena of Nature; by SAMUEL J. METCALF, M.D. 2 vols. 8vo, pp. 630 and 481. Philadelphia: J. B. Lippincott & Co.

Announced.

A Dictionary of English Surnames; by JOHN HENRY ALEXANDER, Esq., of Baltimore, Maryland.—The work will be comprehended in about one thousand pages, and it will be sent to press directly after the necessary commercial and technical arrangements can be made.

Little, Brown & Company, of Boston, propose to publish by subscription a series of photo-lithographic plates of the Fossil Footprints found on the Connecticut River Sandstone, prepared by the late Dr. JAMES DEANE, of Greenfield, in one volume, 4to. Price $5.00.—The work will be issued under the superintendence of T. T. BOUVE, Esq., A. A. GOULD, M.D., and HENRY I. BOWDITCH, M.D., and for the benefit of the family of Dr. Deane, and will be published in the best style, similar to Prof. Agassiz's "Contributions to the Natural History of the United States." Two hundred subscribers are required.

OBITUARY.—

Mr. GUSTAVUS WURDEMANN died on the 29th of Sept. 1859 at Swedesboro, N. J., aged 41 years. Mr. Wurdemann was employed in the U. S. Coast Survey since 1837, in the last twelve years of his life principally as a tidal and meteorological observer in Florida and the Gulf of Mexico. The observations made by him are of great value for their completeness and faithfulness. The short intervals of time left to him by the confining nature of his duties he employed with much success in collecting objects of natural history, and as he was mostly stationed on parts of our coast seldom visited by naturalists, he succeeded in obtaining several species new to science and still more which were new to the fauna of the United States. His collections are in the museums of the Smithsonian Institution and of Prof. Agassiz in Cambridge. Most of his zoological acquisitions have been already published to the world. The largest of our North American herons, *Ardea Wurdemanni*, was discovered by him.

JAMES P. ESPY, one of the most successful meteorologists of our time, died in Cincinnati, on the 24th of January, after a short illness, in the 75th year of his age. We expect to present a notice of his life in our May number.

JEAN-FRÉD.-LUDW. HAUSMANN, the eminent mineralogist, died at Göttingen, Dec. 26, 1859, aged 77 years 10 months.